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Glimpses of the Cosmos

A Mental Autobiography

By Lester F. Ward, LL.D.

- Vol. I. Adolescence to Manhood Period, 1858–1871 Age, 16–30
- Vol. II. Scientific Career Inaugurated. Period, 1875–1882. Age, 34–40
- Vol. III. Dynamic Sociology. Period, 1882–1885. Age, 40–44
- Vol. IV. Period, 1885–1893. Age, 44–52
- Vol. V. Period, 1893–1897. Age, 52–55
- Vol. VI. Period, 1897–1912. Age, 56–70

GLIMPSES OF THE COSMOS

BY

LESTER F. WARD

COMPRISING HIS MINOR CONTRIBUTIONS
NOW REPUBLISHED, TOGETHER WITH
BIOGRAPHICAL AND HISTORICAL
SKETCHES OF ALL HIS WRITINGS

> The writings by which one
> can live are not the writings
> which themselves live.—JOHN
> STUART MILL.

Portrait of the Author

At the age of 61

GLIMPSES OF THE COSMOS

BY

LESTER F. WARD

VOLUME VI

PERIOD, 1897–1912. AGE, 56–70

> Man's works alone suggest design.—
> ASA GRAY.

> Le progres est le developpement de l'ordre.—
> AUGUSTE COMTE.

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NOTE

In the publishers' note to Volume IV. of *Glimpses of the Cosmos*, it was stated that the series would be reduced from the original twelve volumes planned by the author, to eight volumes. It has since been found feasible to reduce the size of the series still more. With the issue of this, the sixth volume, *Glimpses of the Cosmos* is completed.

This condensation has been made by the omission of many of the lesser of Dr. Ward's minor writings. The autobiographical matter remains untouched; it is given just as it came from the hand of the author.

363809

Social betterment. Meliorism.

> I don't know that I ever heard anybody use the word "meliorist" except myself. But I begin to think that there is no good invention or discovery that has not been made by more than one person. The only good reason for referring to the "source" would be that you found it useful for the doctrine of meliorism to cite one unfashionable confessor of it in the face of the fashionable extremes.—George Eliot.

> In her general attitude towards life George Eliot was neither optimist nor pessimist. She held to the middle term, which she invented for herself, of "meliorist."—J. W. Cross.

Historical view of religion. Religion as a system of social control.

> So conspicuous are the proofs that among unallied races in different parts of the globe, progress in civilization has gone along with development of a religious system, absolute in its dogmas and terrible in its threatened penalties, administered by a powerful priesthood, that there seems no escape from the inference that the maintenance of social subordination has peremptorily required the aid of some such agency.—Herbert Spencer.

The transition from a pain economy to a pleasure economy.

> In describing the leading features of a pain economy, I do not mean to imply that men in such an economy are constantly thinking of pain and never of pleasure, but that all their institutions have as their basis the fear of enemies and pain. . . . We are now in the transition stage from this pain economy to a pleasure economy.—Simon N. Patten.

> The greatest possible happiness of the greatest possible number. Maximization of pleasures, minimization of pains.—Jeremy Bentham.

Geology of the Black Hills. Petrified trunks of Cycads and Sequoias.

> The forests were of solid stone, and so hard that they shivered to pieces the finest-tempered axes with which we endeavored to cut them down. . . . This account, at first discredited, has since been corroborated by the discovery of a completely petrified forest near the head waters of the Chayenne, or Chienne River, which has its source in the Black Hills of the Rocky chain.—EDGAR ALLAN POE.

System of sociology planned and largely executed. Sociology reduced to an exact science. Social mechanics. Social statics and social dynamics. The formation and transformation of cosmic, organic, and social structures.

Formative principles.
Synergy

> Les trois lois fondamentales de la mécanique offrent, en elles-mêmes, un puissant intérêt philosophique. . . . Le positivisme représente chacune d'elles comme le germe nécessaire d'une plus grande loi qui convient à tous les phénomènes d'activité. . . . Son entière extension mécanique conduit au célèbre principe construit par d'Alembert, pour ramener l'étude du mouvement d'un système quelconque à celle de l'équilibre correspondant.—AUGUSTE COMTE.

> La force vitale . . . soutient une lutte perpetuelle contre les forces auxquelles obéissent les corps inanimés, et la vie n'est que ce combat prolongé entre ces deux forces différentes. —JEAN LAMARCK.

Creative synthesis.

> Es gibt absolut kein solches Gebilde das nicht nach der Bedeutung und dem Werth seines Inhaltes mehr wäre als die blosse Summe seiner Factoren oder die blosse mechanische Resultante seiner Componente.—WILHELM WUNDT.

Transformative principles.

Difference of potential.

> Le travail des forces, l'énergie kynétique du système sera égale à la différence des énergies potentielles. . . . Pour que la transformation en forces actives des énergies biologiques latentes potentielles ait lieu, il faut qu'entre les races comprises dans un agrégat social, il existe une différence. . . . Toute la différence de ces énergies potentielles passe en énergie kynétique—mais la somme totale d'énergie reste invariable pendant la transformation; il n'y a qu' un changement de forme.—LÉON WINIARSKI.

Conation

> When a man is perfectly content with the state he is in, which is when he is perfectly without any uneasiness, what industry, what action, what will is there left?—JOHN LOCKE.

> Les efforts nécessaires pour atteindre à la vérité sont plus féconds et bienfaisants que la vérité elle-même. Eh bien, disons-en autant des efforts de l'homme pour réaliser la justice sur la terre; même s'ils n'atteignent pas leur but, ils arrivent à un résultat encore plus précieux, l'élévation du caractère individuel.—ACHILLE LORIA.

The androcratic period. The subjection of Woman.

> The inequality of rights between men and women has no other source than the law of the strongest.—John Stuart Mill.

Universality and versatility of genius.

> Genius as an explosive power beats gunpowder hollow; and if knowledge, which should give that power guidance, is wanting, the chances are not small that the rocket will simply run amuck among friends and foes.—Thomas Huxley.

> Only when genius is married to science can the highest results be produced.—Herbert Spencer.

> Le génie est dans les choses et non dans l'homme. L'homme n'est que l'accident qui permet au génie de se dégager.—Alfred Odin.

> ———— man is not Man as yet.
> Nor shall I deem his object served, his end
> Attained, his genuine strength put fairly forth,
> While only here and there a star dispels
> The darkness, here and there a towering mind
> O'erlooks its prostrate fellows: when the host
> Is out at once to the despair of night,
> When all mankind alike is perfected,
> Equal in full-blown powers—then, not till then,
> I say, begins man's general infancy.
> —Robert Browning.

Rise of the proletariat. Progress through the spread of knowledge to the masses. Knowledge of the environment.

> The totality of human actions is governed by the totality of human knowledge.—Thomas Buckle.

> The welfare of mankind primarily depends on correct knowledge of the world in which we live and of human nature.—William Graham Sumner.

Contents of Volume VI.

NO. PAGE

437. What are Stipules? Review of The Nature and Origin of Stipules, by A. A. Tyler, A.M. Presented to the Faculty of Pure Science of Columbia University in Partial Requirement for the Degree of Doctor of Philosophy. Annals of the New York Academy of Sciences, Vol. X., New York, 1897, pp. 1–49, pl. i–iii. Also Separate: Contributions from the Department of Botany of Columbia University, No. 119 1

438. Review of Organisme et Société. Par René Worms. Bibliothèque Sociologique Internationale, I. Paris: V. Giard et E. Brière, 1896. Pp. 412; and La Pathologie Sociale. Par Paul de Lilienfeld. Avec une Preface de René Worms. Bibliothèque Sociologique Internationale, II. Paris: V. Giard et E. Brière, 1896. Pp. xlvii 332 2

439. The Nature of Pleasure 4

440. The Cretaceous Formation of Southwestern Kansas 6

441. [Announcement of Outlines of Sociology] . . 7

442. [The Mesquite in Southern Kansas] . . 8

NO. PAGE

443. ADMINISTRATIVE REPORT FOR THE YEAR ENDING JUNE 30, 1896 9

444. THE ESSENTIAL NATURE OF RELIGION. . . 9

445. ["THE INCREASE AND DIFFUSION OF KNOWLEDGE AMONG MEN" 33

446. A NEW SPECIES OF EUCALYPTUS FROM THE DAKOTA GROUP OF SOUTHWESTERN KANSAS . . 34

447. BIBLIOGRAPHY FOR THE YEAR ENDING JUNE 30, 1895 34

448. PSORALEA TENUIFLORA AS A TUMBLE-WEED . 36

449. LITERARY NOTE [OUTLINES OF SOCIOLOGY] . . 36

450. [DR. SCHENK'S METHOD OF CONTROLLING SEX] . 37

451. UTILITARIAN ECONOMICS 38

452. OUTLINES OF SOCIOLOGY 41

453. REVIEW OF THE SOCIAL MIND AND EDUCATION. BY GEORGE EDGAR VINCENT. PRICE $1.25. PP. IX, 156. NEW YORK: THE MACMILLAN COMPANY, 1897. 44

454. L'ÉOCNOMIE DE LA DOULEUR ET L'ÉCONOMIE DU PLAISIR 45

455. A NEW COMPASS PLANT 47

456. REVIEW OF FOSSIL PLANTS FOR STUDENTS OF BOTANY AND GEOLOGY. BY A. C. SEWARD, M.A., F.G.S. WITH ILLUSTRATIONS. VOL. I, CAMBRIDGE, 1898. PP. XVIII+452. CAMBRIDGE NATURAL SCIENCE MANUALS. BIOLOGICAL SERIES . 48

457. DESCRIPTIONS OF THE SPECIES OF CYCADEOIDEA, OR FOSSIL CYCADEAN TRUNKS, THUS FAR DETER-

NO. PAGE

MINED FROM THE LOWER CRETACEOUS RIM OF THE BLACK HILLS 49

458. REVIEW OF TRUTH AND ERROR, OR THE SCIENCE OF INTELLECTION. BY J. W. POWELL. CHICAGO, THE OPEN COURT PUBLISHING COMPANY . . 51

459. ADMINISTRATIVE REPORT FOR THE YEAR ENDING JUNE 30, 1897 55

460. SOCIOLOGY AND ECONOMICS 55

461. THE GOSPEL OF ACTION 56

462. ADMINISTRATIVE REPORT FOR THE YEAR ENDING JUNE 30, 1898 63

463. THE CRETACEOUS FORMATION OF THE BLACK HILLS AS INDICATED BY THE FOSSIL PLANTS . . . 64

464. THE FOSSIL FORESTS OF ARIZONA 71

465. DESCRIPTION OF A NEW GENUS AND TWENTY NEW SPECIES OF FOSSIL CYCADEAN TRUNKS FROM THE JURASSIC OF WYOMING 73

466. REVIEW OF SOCIAL LAWS. AN OUTLINE OF SOCIOLOGY. BY G. TARDE. TRANSLATED FROM THE FRENCH BY HOWARD C. WARREN, ASSISTANT PROFESSOR OF EXPERIMENTAL PSYCHOLOGY IN PRINCETON UNIVERSITY, WITH A PREFACE BY JAMES MARK BALDWIN. NEW YORK, THE MACMILLAN COMPANY, 1899. SMALL 8°. PP. XII+213 74

467. REPORT ON THE PETRIFIED FORESTS OF ARIZONA . 76

468. [APPRECIATION OF THE MONIST AND OPEN COURT] 99

469. REVIEW OF THE THEORY OF THE LEISURE CLASS. AN ECONOMIC STUDY IN THE EVOLUTION OF

NO. PAGE

Institutions. By Thorstein Veblen. New York: The Macmillan Co., 1899. Pp. viii +400, 8vo 100

470. Administrative Report for the Year Ending June 30, 1899 101

471. Status of the Mesozoic Floras of the United States, First Paper 102

472. Elaboration of the Fossil Cycads in the Yale Museum 107

473. The Autochthonous or Allochthonous Origin of the Coal and Coal Plants of Central France 109

474. Administrative Report for the Year Ending June 30, 1900 111

475. Review of a Memoir Sur Quelques Microorganismes des Combustibles Fossiles. Par B. Renault. One vol. roy 8vo, pp. 460, with 66 text figs.; atlas, folio, 30 plates with Explanation Sheets. Extrait du Bulletin de la Société de l'Industrie Minérale. Troisième série, tome XIII, 4^{e} livraison, 1899. Tome XIV, 4^{e} livraison, 1900. Saint-Etienne. 1900 111

476. La Mécanique Sociale 112

477. [Discussion of La Mécanique Sociale] . . 115

478. Geology of the Little Colorado Valley . . 116

479. Administrative Report for the Year Ending June 30, 1901 116

480. Contemporary Sociology 117

481. Sociology at the Paris Exposition of 1900 . . 121

482. [Definition of Sociology] 126

NO. PAGE
483. THE WORD "ECOLOGY" 127
484. [LE MATÉRIALISME HISTORIQUE] 128
485. SOCIOLOGY AS A SCIENCE 128
486. REVIEW OF HOMERIC SOCIETY. A SOCIOLOGICAL STUDY OF THE ILIAD AND ODYSSEY. BY ALBERT GALLOWAY KELLER, PH.D. NEW YORK AND LONDON: LONGMANS, GREEN & CO., 1902 . 137
487. PURE SOCIOLOGY. A TREATISE ON THE ORIGIN AND SPONTANEOUS DEVELOPMENT OF SOCIETY . . 138
488. ADMINISTRATIVE REPORT FOR THE YEAR ENDING JUNE 30, 1902 154
489. SOCIAL DIFFERENTIATION AND SOCIAL INTEGRATION . 154
490. LA DIFFERENCIATION ET L'INTÉGRATION SOCIALES—UNE UTOPIE SOCIOLOGIQUE 156
491. [EHRENBERG'S SCIENTIFIC PREDICTION]. . . 158
492. THE DRESDEN CYCAD 159
493. [HERBERT SPENCER, HIS DEATH, HIS VISIT TO WASHINGTON, SOME OF HIS LAST LETTERS, SKETCH OF HIS CAREER] 160
494. ADMINISTRATIVE REPORT FOR THE YEAR ENDING JUNE 30, 1903 165
495. [NOTE ON THE DEGREE OF OFFICER OF PUBLIC INSTRUCTION OF FRANCE, CONFERRED] . . . 165
496. REVIEW OF THE POSITIVE PHILOSOPHY OF AUGUSTE COMTE. BY L. LÉVY-BRUHL. AUTHORIZED TRANSLATION, TO WHICH IS PREFIXED AN INTRODUCTION BY FREDERIC HARRISON. NEW YORK, G. P. PUTNAM'S SONS. 1903. PP. XIV + 363. 8° 166

NO. PAGE

497. MONOGENISM AND POLYGENISM 168

498. HERBERT SPENCER'S SOCIOLOGY 169

499. REVIEW OF WHERE DID LIFE BEGIN? A BRIEF INQUIRY AS TO THE PROBABLE PLACE OF BEGINNING AND THE NATURAL COURSES OF MIGRATION THEREFROM OF THE FLORA AND FAUNA OF THE EARTH. A MONOGRAPH. BY GILBERT HILTON SCRIBNER. NEW EDITION. NEW YORK: CHARLES SCRIBNER'S SONS, 1903 178

500. HERBERT SPENCER'S AUTOBIOGRAPHY 179

501. [THE RISE OF THE PROLETARIAT] 188

502. DISCOURS D'OUVERTURE 190

503. [L'ORGANISME SOCIAL] 191

504. LES RAPPORTS ENTRE LA SOCIOLOGIE ET LA PSYCHOLOGIE 191

505. DISCOURS DE CLÔTURE 193

506. THE PTERIDOSPERMAPHYTA 194

507. A FAMOUS FOSSIL CYCAD 194

508. PALEOZOIC SEED PLANTS 195

509. [DEATH OF GABRIEL TARDE] 196

510. [MONUMENT TO BERNARD RENAULT] 198

511. REVIEW OF THE JURASSIC FLORA. BY A. C. SEWARD, F.R.S. PART I, THE YORKSHIRE COAST. LONDON, 1900. PART II, LIASSIC AND OOLITIC FLORAS OF ENGLAND (EXCLUDING THE INFERIOR OOLITE PLANTS OF THE YORKSHIRE COAST), LONDON, 1904. CATALOGUE OF THE MESOZOIC PLANTS IN THE DEPARTMENT OF GEOLOGY, BRITISH MUSEUM (NATURAL HISTORY). PARTS III, IV 199

NO. PAGE

512. ADMINISTRATIVE REPORT FOR THE YEAR ENDING JUNE 30, 1904 200

513. LE SUJET-MATIÈRE DE LA SOCIOLOGIE . . . 200

514. AN EXAMPLE IN NOMENCLATURE 200

515. EVOLUTION OF SOCIAL STRUCTURES . . . 201

516. MENTHACEAE 206

517. REVIEW OF AN OUTLINE OF THE THEORY OF ORGANIC EVOLUTION, WITH A DESCRIPTION OF SOME OF THE PHENOMENA WHICH IT EXPLAINS. BY MAYNARD M. METCALF. NEW YORK: THE MACMILLAN COMPANY. 1904. 8°, XXII+204 PAGES, ILLUSTRATED 208

518. A TEXT-BOOK OF SOCIOLOGY. BY JAMES QUAYLE DEALEY, PH.D., PROFESSOR OF SOCIAL AND POLITICAL SCIENCE IN BROWN UNIVERSITY, AND LESTER F. WARD, LL.D., OF THE SMITHSONIAN INSTITUTION AT WASHINGTON, D. C. NEW YORK. THE MACMILLAN COMPANY, 1905 . . 209

519. STATUS OF THE MESOZOIC FLORAS OF THE UNITED STATES. SECOND PAPER 213

520. LES FORCES PHYLOGÉNÉTIQUES 220

521. [ANNOUNCEMENT OF APPLIED SOCIOLOGY] . . 221

522. THE PAST AND FUTURE OF THE SEXES . . . 223

523. APPLIED SOCIOLOGY.—A TREATISE ON THE CONSCIOUS IMPROVEMENT OF SOCIETY BY SOCIETY . 225

524. [SECOND ANNOUNCEMENT OF APPLIED SOCIOLOGY] 236

525. [ANNOUNCEMENT OF THE PSYCHIC FACTORS OF CIVILIZATION, SECOND EDITION] 239

526. [NOTE ON SYMPODIAL PLANTS] 241

NO. PAGE

527. THE ESTABLISHMENT OF SOCIOLOGY.—ABSTRACT . 242

528. REVIEW OF SOCIOLOGY AND SOCIAL PROGRESS: A HANDBOOK FOR STUDENTS OF SOCIOLOGY. COMPILED BY THOMAS NIXON CARVER, PH.D., LL.D., DAVID A. WELLS PROFESSOR OF POLITICAL ECONOMY IN HARVARD UNIVERSITY. BOSTON, GINN AND COMPANY [1906]. PP. VI+810; 8°. LIST PRICE, $2.75; MAILING PRICE, $2.95 . . 243

529. MARS AND ITS LESSON 244

530. THE ESTABLISHMENT OF SOCIOLOGY . . . 259

531. THE FINE ARTS AS A DYNAMIC FACTOR IN SOCIETY . 266

532. SOCIAL DARWINISM 268

533. LES LUTTES SOCIALES ET LES LUTTES BIOLOGIQUES . 269

534. LA SOCIOLOGIE DES PARTIS POLITIQUES . . . 272

535. SOCIAL AND BIOLOGICAL STRUGGLES . . . 273

536. RELATIONS OF THE SOCIAL SCIENCES.—A SYMPOSIUM 274

537. WHAT BRINGS OUT GENIUS? IS IT INHERITANCE, OPPORTUNITY, TRAINING, ENVIRONMENT, OR LUCK 277

538. SOCIAL CLASSES IN THE LIGHT OF MODERN SOCIOLOGICAL THEORY.—ABSTRACT 291

539. THE SOCIOLOGY OF POLITICAL PARTIES . . . 292

540. SOCIAL CLASSES IN THE LIGHT OF MODERN SOCIOLOGICAL THEORY 293

541. MOTIVES IN SOCIAL CONFLICT 294

542. [SOCIAL INTEGRATION THROUGH CONFLICT] . . 295

NO. PAGE

543. Review of Social Psychology: An Outline and Source Book: By Edward Alsworth Ross. New York, The Macmillan Company, 1908. Pp. xviii + 372 296

544. The Value of Error 300

545. The Career of Herbert Spencer . . . 301

546. The Status of Sociology 317

547. Review of "The Problem of Age, Growth, and Death. A Study of Cytomorphosis, based on Lectures at the Lowell Institute, March, 1907." By Charles S. Minot. New York and London. G. P. Putnam's Sons, 1908. Pp. xxii, 280. The Science Series . . . 319

548. [Notice of the Seventh Congress of the International Institute of Sociology] . . . 320

549. The Geneva Jubilee 321

550. Ludwig Gumplowicz 325

551. Education and Progress 330

552. Spain and Ferrer 341

553. Sociology and the State 350

554. The Historical View of Woman . . . 353

555. [Solidarité Sociale] Paroles 357

556. [War and Progress] 359

557. [The Hierarchy of the Sciences] . . . 360

558. [Note on a Proposed trip to Christiania, Hamburg and Rome] 361

559. Review of "The Changing Chinese, Oriental and Western Cultures in China." By Ed-

NO. PAGE

WARD ALSWORTH ROSS, PH.D., LL.D., PROFESSOR OF SOCIOLOGY IN THE UNIVERSITY OF WISCONSIN. NEW YORK, THE CENTURY CO. 1911. 8°, PP. XVI+356 362

560. THE STRUGGLE OF TO-DAY 364

561. APPRECIATION OF SOCIOLOGICAL STUDY OF THE BIBLE, BY LOUIS WALLIS 370

562. DÉFINITION DU PROGRÈS SOCIAL 372

563. EUGENICS, EUTHENICS, AND EUDEMICS . . . 382

NUMERICALLY CLASSIFIED CONTENTS

The objections to classifying the subjects of my lesser published writings were discussed in the preface (supra, Vol. I, p. xii), but the desirability of some such classification is nevertheless apparent. There may, for example, be readers who would like to read all I have written on some particular subject or science, and do not wish to be put to the trouble of looking through the extended list and trying to select those relating to that subject. To meet such cases I have prepared this list of subjects or sciences and given after each all the numbers of articles bearing upon them. For the reasons stated, this is quite difficult, but where an article covers several sciences I have repeated the numbers under each. At least half the numbers thus appear under two or more sciences. I have also distinguished reviews and notices of books, as well as official reports, but all such occur again under the appropriate subjects.

The arrangement is that of the serial classification of the sciences as given in the first two columns of the Tabular View of All Knowledge, Vol. V, No. 373, and in the ascending order, but the number of subsciences is considerably increased and made to include certain special subjects on which I have written, and which I have endeavored to class under their appropriate general sciences. I have always maintained that, as this includes all knowledge, there cannot be a subject which is incapable of being placed under some of the heads. The only qualification that this requires is that the subject must relate to real things. The two truly abstract sciences, mathematics and logic, which deal with relations only, the first with quantitative, and the second with qualitative relations, are simply norms, and do not fall into any scheme of real things. I have not written on formal logic, though I have discussed it in several of my books, but a few of my papers may be incidentally classed under mathematics. This I place, as did Comte, in front of the whole series. Philosophy may be said to include both quantitative and qualitative relations, but it also covers all other relations, and most of the papers that I class under *philosophy* deal with concrete sciences as well, and the numbers will also be found under these; but I thought best to place all such under the general head of philosophy. As this obviously cannot be brought under any one of the sciences proper, its only place seems to be at the head of the list, and I have so assigned it.

I have not written on any branch of physics, but have had occasion to deal with the great physical laws of the universe. This I call *cosmology*, and put it in the place physics would have occupied. Nothing else needs explaining till the special social sciences are reached, falling under sociology. A few of these not in the table are introduced. Under *philology*, which includes everything relating to language, whether spoken or written, I have placed certain by-products which might seem to belong higher in the list, such, namely, as letters from the army to a friend, my personal bibliography, reviews and press notices, official reports, and even biographical sketches. The only important deviation from the table here is the transposition of *gamology* from sociology to biology. So many of my papers deal with the sex relations of the lower animals as well as man, and especially of plants, that it seemed proper to make the term apply to all, and not merely to the human race. The word *gamology* is not of my coining. It was apparently first used by a French publicist, M. Charles M. Limousin, in a communication to the Société Sociologique de Paris on June 14, 1905, and appears in the *Revue Internationale de Sociologie* for July, 1905, Vol. XIII, p. 545. The etymology is perfect and the term was much needed for all that relates to marriage and to the relations of the sexes in general.

A uniform terminology for all the sciences* seems impossible, but in the table I have made as many of them as possible end in *-ology*, as indicating a treatise on or treatment of the several subjects of them, and, besides gamology, I have introduced a few other unfamiliar ones for the sake of uniformity. *Nomology* and *politology* are fully justified, because there is no single English word for the science of either law or the state. *Ecology*, of course, as introduced by Haeckel in 1866, for the relations of the organism to its environment, has passed fully into biology, but has been badly perverted. Its broader use here may be accepted without affecting the earlier and narrower meaning. Finally *ethology*, so improperly applied by John Stuart Mill to the science of character, and still more improperly by modern biologists to ecology in Haeckel's sense, has been restored by Wundt* to its proper meaning, as I use it. This table was prepared and enlarged as a chart for use in my classes two years before the appearance of Professor Sumner's *Folkways*, in which, on pages 36, 37, and 561, and apparently without knowing that Wundt had already done so, he makes a strong plea for its use in this original and etymologically correct sense.

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* See *Outlines of Sociology*, pp. 139–140.

In ranging the subsciences under the general sciences, no attempt is here made, by indentation or otherwise, to indicate their subordination. That is sufficiently apparent from their position. But in certain cases where the name of the science is unfamiliar the older and better-known name or names are made to follow the new name in parentheses.

PHILOSOPHY, 27, 30, 48, 83, 89, 109, 114, 120, 135, 137, 145, 219, 220, 231, 238, 349, 361, 373, 374, 387, 389, 390, 400, 401, 418, 436, 444, 452, 458, 466, 468, 476, 477, 480, 487, 496, 503, 529, 546, 557.

MATHEMATICS, 131, 231, 418, 476.

ASTRONOMY, 109, 529.

METEOROLOGY, 118, 233, 246.

GEOGRAPHY, 92, 118, 134, 152, 176, 185, 196, 209, 263, 310, 330.

GEOLOGY, 159, 176, 209, 239, 252, 295, 299, 301, 302, 310, 312, 313, 364, 365, 405, 421, 424, 425, 432, 440, 463, 471, 473, 478, 519, 529.

COSMOLOGY, 89, 109, 135, 145, 349, 390, 452, 487.

CHEMISTRY, 109, 136, 154.

BIOLOGY, 78, 83, 84, 85, 89, 103, 110, 115, 118, 126, 134, 136, 154, 155, 168, 199, 214, 240, 241, 253, 265, 290, 291, 292, 318, 331, 348, 366, 392, 394, 408, 414, 415, 438, 450, 483, 491, 499, 503, 517, 532, 533, 535, 548.

PLASMATOLOGY, 136, 154.

GAMOLOGY (Relations of the Sexes, Marriage), 24, 69, 95, 100, 102, 110, 126, 141, 145, 162, 241, 268, 326, 359, 450, 487, 520, 522, 554.

BOTANY, 71, 72, 73, 74, 75, 76, 77, 78, 79, 86, 87, 88, 93, 94, 95, 96, 97, 98, 99, 100, 101, 102, 103, 107, 110, 116, 118, 119, 122, 123, 124, 139, 140, 141, 142, 152, 153, 157, 167, 171, 174, 175, 191, 211, 215, 216, 244, 247, 253, 259, 265, 275, 279, 281, 330, 332, 342, 358, 385, 437, 442, 448, 455, 491, 506, 508, 516, 526.

---

* *Logik*, 2 Aufl., Bd. II, S. 369 ff.

PALEOBOTANY, 142, 149, 158, 159, 165, 170, 172, 180, 183, 184, 185, 186, 187, 188, 191, 192, 193, 194, 195, 204, 205, 207, 208, 213, 215, 216, 222, 223, 224, 227, 229, 232, 235, 236, 239, 242, 243, 245, 248, 249, 251, 252, 256, 257, 258, 261, 262, 263, 264, 266, 267, 269, 270, 271, 272, 273, 274, 275, 276, 277, 278, 279, 280, 284, 285, 286, 287, 288, 289, 295, 296, 297, 299, 300, 301, 302, 304, 305, 306, 307, 308, 309, 312, 313, 316, 317, 319, 320, 321, 322, 323, 324, 327, 328, 332, 333, 335, 336, 337, 338, 339, 340, 344, 347, 352, 353, 362, 364, 365, 368, 370, 371, 375, 377, 382, 386, 388, 393, 397, 404, 405, 409, 420, 421, 424, 425, 426, 432, 433, 440, 443, 446, 456, 457, 459, 462, 463, 464, 465, 467, 470, 471, 472, 473, 474, 475, 479, 488, 491, 492, 494, 506, 507, 508, 511, 512, 514, 519.

ZOOLOGY, 84, 85, 108, 115, 134.

ENTOMOLOGY, 103, 190.

ORNITHOLOGY, 82, 385.

PSYCHOLOGY, 132, 133, 179, 182, 198, 210, 268, 331, 343, 346, 349, 350, 351, 356, 357, 373, 374, 396, 402, 430, 434, 439, 453, 458, 466, 504, 537, 544.

SOCIOLOGY, 69, 90, 91, 92, 104, 105, 106, 111, 113, 127, 132, 137, 143, 145, 146, 147, 148, 151, 156, 163, 164, 169, 177, 178, 179, 181, 197, 198, 200, 201, 212, 214, 218, 221, 228, 241, 250, 255, 268, 283, 290, 291, 293, 294, 303, 311, 325, 326, 331, 334, 343, 345, 346, 349, 350, 351, 355, 359, 363, 369, 373, 374, 378, 379, 381, 383, 384, 387, 390, 391, 392, 395, 396, 399, 401, 402, 403, 406, 407, 410, 411, 412, 413, 416, 417, 418, 419, 422, 423, 428, 429, 430, 431, 434, 435, 438, 441, 444, 449, 450, 451, 452, 453, 454, 458, 460, 461, 466, 469, 476, 477, 480, 481, 482, 484, 485, 486, 487, 489, 490, 497, 498, 501, 502, 503, 504, 505, 513, 515, 518, 520, 521, 522, 523, 524, 525, 527, 528, 530, 531, 532, 533, 534, 535, 536, 537, 538, 539, 540, 541, 542, 543, 544, 545, 546, 547, 550, 551, 553, 554, 555, 556, 559.

ANTHROPOLOGY, 85, 104, 111, 112, 128, 130, 131, 134, 138, 162, 182, 197, 210, 254, 384, 399, 489, 490, 497, 545.

ETHNOLOGY, 134, 138, 162.

DEMOGRAPHY, 54, 90, 132, 228, 429, 501, 532, 538, 540, 551, 559.

IMMIGRATION, 54.

STATISTICS, 54, 90.

PHILOLOGY (Language, Literature, etc.), 83, 112, 125, 129, 130, 133, 150, 161.

FICTION, 1.

LETTERS FROM THE ARMY, 3, 4.

BIBLIOGRAPHY, 144, 145, 166, 189, 206, 217, 260, 282, 298, 329, 349, 354, 372, 398, 427, 447, 515, 518, 523.

REVIEWS, 47, 156, 186, 187, 230, 232, 238, 297, 318, 360, 367, 389, 394, 400, 403, 407, 408, 409, 410, 412, 414, 415, 419, 423, 435, 436, 437, 438, 453, 456, 458, 466, 469, 475, 486, 496, 499, 500, 511, 517, 528, 544, 546, 548, 559.

PRESS NOTICES, 143, 146, 147, 148, 151, 229, 235, 236, 242, 256, 257, 261, 264, 269, 270, 271, 272, 273, 274, 276, 277, 284, 285, 286, 287, 288, 289, 304, 305, 306, 307, 308, 309, 316, 317, 319, 320, 321, 322, 323, 324, 325, 335 336, 337, 338, 339, 340, 416, 468.

BIOGRAPHY, 117, 122, 145, 226, 240, 333, 386, 414, 493, 495, 498, 500, 509, 510, 515, 522, 546, 550, 558.

OBITUARIES, 45, 341, 380, 509, 550.

ETHNOLOGY (Custom, Mores, Ethics), 43, 115, 164, 201, 212, 334, 355, 396, 423, 439, 444, 451, 454.

THEOLOGY (Religion, Mythology, Magic, Superstition, Exegesis, Biblical Criticism), 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 16, 17, 18, 19, 20, 21, 22, 23, 24, 25, 26, 27, 28, 29, 30, 31, 32, 33, 34, 35, 36, 37, 38, 39, 40, 41, 42, 43, 44, 46, 48-49, 50, 51, 52, 53, 55, 56, 57, 58, 59, 60, 61, 62, 63, 64, 65, 66, 67, 68, 69, 70, 160, 182, 219, 220, 225, 230, 238, 254, 383, 436, 444, 545.

ECOLOGY (Economics, Political Economy), 113, 121, 218, 228, 250, 255, 283, 293, 331, 360, 378, 379, 391, 451, 454, 460, 469, 484.

POLITOLOGY (Political Science, Politics, Government, State), 5, 80, 81, 113, 121, 127, 173, 177, 178, 181, 202, 221, 237, 255, 294, 311, 314, 334, 345, 355, 367, 411, 534, 539, 552, 553.

NOMOLOGY (Law, Legislation, Jurisprudence), 34, 42, 44, 91.

ESTHETOLOGY (The Fine Arts), 531.

PEDAGOGY (Education), 2, 145, 203, 234, 290, 350, 445, 453, 481, 495, 523, 549, 551, 552.

NUMERICAL BIBLIOGRAPHY

It has already been noted (supra, Vol. I, pp. i–xx) that one of the strong reasons for trying to bring out my lesser published writings in an accessible form was the great number of different periodicals and places in which they originally appeared, some of them very difficult to find. While I always knew where my writings were published, I had never made any attempt to get these original sources into any convenient form for consultation. I had no clear idea as to how many such sources there were, and had never made a list of them in order to find out. In the present work the reference to the original place of publication always stands immediately before the article following the historical sketch, where it may be easily consulted. What seemed to be needed was some condensed view of all these references. But to draw up such a list and let each reference, journal, or book in which the articles had been published follow would occupy much space, and seemed to be needless. The numerical system adopted, however, makes it possible to accomplish the same object in very compact form, as the numbers occupy little space, and therefore a list of all the publications followed by the numbers is not a serious matter, and must have a special value.

I therefore decided to introduce this feature, and I went carefully through the entire manuscript and transferred the titles of all periodicals and other sources to cards, and let them be followed by the numbers. I numbered these cards in the order of the first occurrence of an article in any periodical, so that when I was through the last number would represent the total number of all sources. This proved to be No. 123. But the sources are very heterogeneous, and as this numerical bibliography is to embrace all my writings of every kind, I have included many things besides regular periodicals and serials. For example, it must include my six books, and a number of other independent publications, such as circulars, announcements of books written by myself, pamphlets not appearing elsewhere, and at least one official report wholly my own and not included in any more general report. Then I have made a good many contributions to books, pamphlets, etc., of others. There are still other miscellaneous sources. The periodicals may be divided into several classes, as newspapers, journals published weekly or fortnightly and having the folio format, magazines published monthly, bimonthly or quarterly, scientific serials, proceedings, transactions, bulletins, etc., of scientific societies, and official reports usually published annually. There are also official serials, called bulletins, monographs, etc., which are not periodical. There are some journals that have undergone such complete transformation without changing their names that I have given them separate numbers. Such is the journal *Science*, the original series of which, started in New York in 1880 by Mr. John Michels, was totally ignored in the second series published in Cambridge in 1883 and edited by Professor Samuel H. Scudder. And after it declined and virtually died in 1893, and was started as a new series in 1895 by Dr. J. McKean Cattell, it was so entirely changed that nothing remained the same but the name. I contributed to all three of these series, but I treat them as distinct periodicals. I also treat *The Open Court*, "A Fortnightly Journal devoted to the Work of establishing Ethics and Religion upon a Scientific Basis," and edited by Mr. B. F. Underwood, as distinct from *The Open Court*, "a Weekly Journal devoted to the Religion of Science," and edited by Dr. Paul Carus, as two different journals, to both of which I contributed. Other rulings may need explanation, but I will not longer dwell upon such anomalous cases. They are comparatively few in number.

How to arrange all these titles was the most serious problem. Any attempt at an alphabetical arrangement seemed out of the question. Librarians are disposed to go by the place of publication because it lends itself best to the alphabetical system, but many journals are peripatetic, and would require to be placed under several cities. And then it often happens that a journal is printed at some small remote place where there are exceptional facilities for printing, while to all intents and purposes it is published in some large city. *Science*, published by the Macmillan Company of New York, is printed at Lancaster, Pennsylvania, and edited at Garrison, New York. After considering all arguments, therefore, I decided to arrange them in the chronological order of the first number in which an article of mine appeared in each journal or series. This maintains the biographical character of the entire work, and it will not probably be more difficult to find things under this arrangement than it would be under any other that could be adopted.

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2. *The Bradford Argus*, Towanda, Bradford County, Pennsylvania, 2, 3, 4

3. *The Daily National Republican*, Washington, D. C., 5.

4. Circulars and printed tracts, not published in any other way, 6, 233, 468.

5. *The Iconoclast.* Issued monthly by the National Liberal Reform League. Washington, D. C., 7–53, 57–65, 67–70.

6. *Special Report on Immigration;* by Edward Young, Chief of the Bureau of Statistics, Washington, D. C., 1871, 54.

7. *The Daily Morning Chronicle*, Washington, D. C., 55, 56, 66, 72.

8. *The Semi-weekly Tribune*, New York, 71.

9. *The Daily Tribune*, New York, 71, 80, 81.

10. *Field and Forest.* [A Monthly Journal.] Devoted to General Natural History. Bulletin of the Potomac-Side Naturalists' Club, Washington, D. C., 73, 74, 79, 82, 84, 86, 87, 88, 94, 98, 99.

11. *The American Naturalist*, Salem, Boston, Philadelphia, 75, 77, 93, 97, 102, 110, 136, 167, 195, 232, 275.

12. *Virginia Medical Monthly*, Richmond, Virginia, 76.

13. *The Popular Science Monthly*, New York, 78, 89, 109, 117, 143, 176, 361, 366, 416, 485, 546.

14. *The Evolution*, New York, 83.

15. *The Penn Monthly*, Philadelphia, 85, 113.

16. Brochures either not otherwise published or having a different form from the originals, 85, 96, 119, 357, 467, 480, 551.

17. *The National Union*, Daily Issue, Washington, D. C., 90, 91, 92.

18. *The National Union*, Weekly Issue, Washington, D. C., 91.

19. *The Gardner's Monthly and Horticulturist*, Philadelphia, 95.

20. *Scientific American Supplement*, New York, 100.

21. *Report on the Lands of the Arid Region*, by J. W. Powell, Washington, D. C., 101.

22. *The American Entomologist*, New Series, New York, 103.

23. *The Daily* [Evening] *Critic*, Washington, D. C., 104, 146.

24. *The Boston Advertiser*, 105.

25. *Science:* A Weekly Record of Scientific Progress, New York, 106.

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28. *Abstract of Transactions of the Anthropological Society of Washington*, Washington, D. C., 111, 112.

29. *The Journal of Speculative Philosophy*, New York, 114, 120.

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31. *The Kansas City Review of Science and Industry*, Kansas City, 118.

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33. *The International Review*, New York, 121.

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95. *The American Journal of Sociology*, Chicago, 384, 387, 390, 392, 399, 402, 403, 406, 413, 418, 422, 423, 428, 430, 434, 438, 451, 469, 480, 489, 515, 530, 531, 532, 535, 536, 539, 540, 541, 543, 550, 553, [563].

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Illustrations

PAGE
PORTRAIT OF THE AUTHOR . . . . *Frontispiece*
At the age of 61.

PORTRAIT OF THE AUTHOR 358
At the age of 69.

GLIMPSES OF THE COSMOS

Glimpses of the Cosmos

July 16, 1897—Ætat. 56.

437. What are Stipules? Review of The Nature and Origin of Stipules, by A. A. Tyler, A. M. Presented to the Faculty of Pure Science of Columbia University in partial requirement for the degree of Doctor of Philosophy. Annals of the New York Academy of Sciences, Vol. X., New York, 1897, pp. 1–49, pl. i.–iii. Also separate: Contributions from the Department of Botany of Columbia University, No. 119

History.—Written June 12–19, 1897. It was, in fact, dictated. The memoir of Mr. Tyler was sent me on June 5, by Dr. Britton with the request that I review it in *Science*. I consented and sent the review to him on June 19, to use as he saw fit.

Science, New Series, New York, Vol. VI, No. 133, July 16, 1897, pp. 100–103.

438. Review of Organisme et Société. Par René Worms. Bibliothèque Sociologique Internationale, I. Paris: V. Giard et E. Brière, 1896. Pp. 412; and La Pathologie Sociale. Par Paul de Lilienfeld. Avec une Preface de René Worms. Bibliothèque Sociologique Internationale, II. Paris: V. Giard et E. Brière, 1896. Pp. xlvii + 332

History.—Written June 15–18, 1897. In a letter dated July 13, 1896, Dr. René Worms informed me that he had sent both these books to the *American Journal of Sociology* and requested me to review them. I wrote to Dr. Small about it and received them from him on November 11, 1896. For some reason I did not read them at once and they lay round all winter. In the spring I took them up and had finished the first of them by June 8, 1897. The other I finished reading on June 15. I then realized that I should review them, and proceeded at once to do so. I was in the field in Kansas until near the end of October, and found my reprints on my return and sent them out. The review made quite a lively stir among the "organicists." I received letters from both the authors with regard to it. That of Senator Lilienfeld was very long, going over all the ground. He pointed out a serious error into which I had fallen. I had not then read his great five-volume work, which he then sent me. In his *Pathologie Sociale*, which I reviewed together with the work of Dr. Worms, he summed up his system rather briefly, and in reading this summary I mis-read the phrase "substance intercellulaire" for "structure intercellulaire." Upon this mistaken reading I based the discussion which occurs in the review. He very properly took me severely to task for this, maintaining that "intercellular substance" is something very different from "intercellular structure," and denying that he attributed any structure whatever to the former. This is not the place to criticise his system, but really his idea of a great quantity of structureless substance in an organism, whether biological or social, is rather absurd. Still this is no excuse for my blunder.

He also denied that he had ever compared nerves to telegraph lines, as I stated, but I referred him to a passage in his book from which I had gathered that meaning, though I might have also been mistaken in that. The passage was ambiguous.

The American Journal of Sociology, Chicago, Vol. III, No. 2, September, 1897, pp. 258–265.

October, 1897—Ætat. 56.

439. The Nature of Pleasure

History.—Written June 27, 1897, as a contribution to a discussion going on in the *International Journal of Ethics* between Dr. Patten and Professor Caldwell, neither of whom seemed to me to touch the solid ground of the matter.

International Journal of Ethics, Philadelphia, Vol. VIII, No. 1, October, 1897, pp. 100–101.

DR. PATTEN, in his "Explanation" in answer to Professor Caldwell's comments on his *Theory of Social Forces*, falls into the usual error of most persons who attempt to discuss the utilitarian philosophy. He says that Herbert Spencer's "life of 'unalloyed pleasure' . . . would restrict our activities." He draws a picture of his own of what such a life would be, and says, "Such a hot-house life is the necessary result of the growth of our sensory concepts and of the resulting limitation which knowledge puts on activity." In this he loses sight of the essential nature and only correct definition of pleasure, viz., the normal exercise of the faculties. Pleasure consists entirely of activity. There is nothing "static" about it. The only philosopher who has gone to the bottom of the subject is Schopenhauer, and I am surprised that such a student of Schopenhauer as Professor Caldwell should have failed in his *Reply* to clear up this essential point.

The truth is, as Schopenhauer shows, that pleasure consists in the satisfaction of desire, actual or potential; that desire is a special form of pain, as proved by the universal effort to put an end to it, and that it is this putting an end to desire,—*i.e.*, satisfying it, that yields, or rather, constitutes pleasure. But this can only be accomplished through activity. It is this sense of successful effort which yields all the reward, and that life which secures the greatest pleasure is the most active. The most unendurable of all pains is *ennui*,—*Langeweile*, as Schopenhauer has so graphically painted it. This is the coddled life of inactivity that Dr. Patten pictures as one of "unalloyed pleasure." All that Schopenhauer failed to see was that the satisfaction of desire may have *duration*, or at least may consist of such a prolonged series of repeated satisfactions following one another at inappreciably short intervals, that it cannot be distinguished from continuous pleasure. He missed this essential point because in his time experimental psychology was unknown, by which it has since been demonstrated in the laboratory that every psychic phenomenon *occupies time*.*

The failure to perceive this principle was the one vulnerable spot in the utilitarian philosophy, and John Stuart Mill's "paradox of hedonism," which rests on this failure, as does the present reasoning of Dr. Patten, has given the opponents of utilitarianism a vantage ground which its defenders need not and should not surrender.

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* This demonstration constitutes the "refutation of pessimism," as I pointed out in *The Psychic Factors of Civilization*, Chapter XI.

November 26, 1897—Ætat. 56.

440. The Cretaceous Formation of Southwestern Kansas

History.—Written (dictated) November 11, 1897. This paper gave the results of my campaign in Kansas made in September and October. Mr. C. N. Gould was in my party and besides making large collections of fossil plants from the Cheyenne Sandstone we worked out in detail the geological section connecting those beds with the Dakota Group. I made a diagrammatic section of the whole and we decided on the names of the successive divisions. This diagram I enlarged for this occasion and placed on the blackboard. I afterwards sent a copy of it to Mr. Gould to use in preparing a more extended paper, which he published with the title *On a Series of Transition Beds from the Comanche to the Dakota Cretaceous in Southwestern Kansas*, in the *American Journal of Science* for March, 1898, Fourth Series, Vol. V, pp. 169–175. The section on p. 174 of that paper is the one I sent him. The present paper is only a brief abstract of my remarks, which I furnished the Secretary.

Science, New Series, New York, Vol. VI, No. 152, November 26, 1897, pp. 814–815.

December 3, 1897—Ætat. 56.

441. [Announcement of Outlines of Sociology]

History.—Written at the request of the publishers November 28, 1897. What I wrote included both this and the literary note (see infra, No. 449), and they divided it.

OUTLINES OF SOCIOLOGY

BY

LESTER F. WARD, LL.D.

Lecturer on Sociology at Columbian University, Washington, D. C.,
Author of Dynamic Sociology, etc.

This work appeared in serial form as "Contributions to Social Philosophy" in the *American Journal of Sociology* from July, 1895, to May, 1897, and is treated from the advanced standpoint of active discussion on both sides of the Atlantic during recent years.

Object of the work

The object of the work is to furnish a clear idea of the science of sociology, not by means of verbal definitions, but by clearing the ground and removing the entanglements with other cognate or ancillary sciences, and by a succint presentation of its principles as distinguished from its elements.

Plan is two-fold

The plan is, therefore, two-fold: First, to bound the science and mark it off from other sciences which surround it and impinge upon it, showing what these sciences are and how they are related to it; secondly to lay out the science of sociology itself in a definite scheme of laws and principles susceptible of true scientific treatment.

Social Philosophy

Social Science

The first of these tasks, which the author calls "Social Philosophy," and to which he devotes the first six chapters, or just half of the book, is the subject of Part I., while the second, which he distinguishes as "Social Science," and which occupies the latter half of the book, constitutes Part II. The work is, therefore, devoted to the distinguishing of social philosophy from social science, and to the showing of (1) the relation of sociology to other sciences, and (2) its essential nature as science.

THE MACMILLAN COMPANY

NEW YORK BOSTON CHICAGO SAN FRANCISCO

December 8, 1897—Ætat. 56.

442. [The Mesquite in Southern Kansas]

History.—Written for the department of Notes and News in the *Plant World* some time in November, 1897, after having laid the facts before the Biological Society of Washington on November 6.

The Plant World, Binghamton, N. Y., Vol. I, No. 3, December, 1897, p. 48.

IN September last, while operating in southwestern Kansas, near Belvidere, Kiowa county, Mr. C. N. Gould of Southwest Kansas College, Winfield, Kansas, who was in my party, informed me that some nine miles southeast of Belvidere, on Elk Creek, near the northeast corner of Comanche county, he had seen a little thorny bush bearing bean-pods, that no one could name for him. I at once suspected it to be the mesquite, but had never heard of its growing north of Texas, the boundary line of which is some 300 miles south of that point. On the 15th we visited this place, and Mr. Gould pointed out the bush. It was only four feet high, and fruiting profusely. My suspicions were fully sustained, as it proved to be the *Prosopis juliflora*, at least that form of it in which the leaflets are set far apart, and which was called *P. glandulosa* by Torrey, who considered it distinct from the form in Arizona and westward. This is almost certainly the most northern point at which this genus has thus far been found, and it does not occur in any of the books that include only the State of Kansas in their southern range. Its low, stunted character further shows that this little straggler was out of its normal habitat and had reached its extreme northern limit. The spot is about twenty-five miles north of the Kansas–Oklahoma line.

December 23, 1897—Ætat. 56.

443. Administrative Report for the year ending June 30, 1896

History.—Written June 8, 1896. (See supra, Vol. V, No. 420.) This report appeared in December, 1897.

Seventeenth Annual Report of the U. S. Geological Survey, 1895–96, Part I, Washington, D. C., 1896 [1897], pp. 67–68.

December 25, 1897—Ætat. 56.

444. The Essential Nature of Religion

History.—Written June 7, to July 9, 1896. This article embodies one of the clearest and most definite of those "inspirations" of which I have experienced so many, and which I described in *Pure Sociology*, p. 442. The idea had long been working subconsciously, and once, about two months before it came into full view, I had almost given form to it in words. This occurs in my article on *The Mechanics of Society*, which was written from March 26, to April 2, 1896. The passage reads:

> "Religion and the church exist for the protection of society from assumed spiritual beings and to propitiate them. From a highly philosophical point of view they have a far deeper and more recondite function, viz., that of antagonizing the tendency to violate the laws of nature and jeopardize the existence of the race."*

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* *The American Journal of Sociology*, Vol. II., No. 2, September, 1896, pp. 242–243; *Outlines of Sociology*, 1898, p. 173.

That passage strikes the keynote of this article, and after I had written it, it continued to assume larger and larger proportions in my mind. At length the pressure became irresistible and I was simply forced to develop it in its present form. I may say that in a general way such is the history of everything I have ever written. I am not a literary man in the sense that I write as a business or feel that I must always be writing. I usually resist the impulse until I can resist no longer, until the idea has taken complete possession of me and *must* be expressed. I suppose that is what Nietzsche meant when, being asked why he wrote when he was not obliged to, he replied: "I write to get rid of my thoughts." When one is besieged or obsessed by a thought one does in a manner "get rid" of it by writing and publishing it. In harmony with all this I find this entry in my note-book for June 7, 1896:

> "Made a bare commencement on the rough draft of a new article on the *Essential Nature of Religion*, about which I have been thinking a good deal lately."

I had no idea whatever of what I should do with it, how or where I should publish it, or any of the consequences of writing it. Write it I felt I must.

That "rough draft" was finished on June 21. It was then thoroughly revised, and the final draft was begun on June 28.

On July 1, Mr. Duren J. H. Ward wrote to ask me to come to Greenacre-on-the-Piscataqua, Maine, and give one of the conferences there on July 31. I replied that I could speak on the subject of this article. This was agreed to and I went and read my paper on that date. I worked it over a good deal in the interval, adapting it more to the platform, and revised it thoroughly. It was a marked success and the audience was very large and appreciative. I lingered some time in that charming place and made a number of valuable acquaintances. I loaned the manuscript to Miss American, a very intelligent lady. She of course returned it after reading it.

On January 11, 1897, I sent the final draft of the article to Dr. Carus and offered it to him for the *Monist*. He wrote me a long letter about it before he had finished reading it, saying that he would publish it, but that he did not wholly agree with it and wanted to write an article on the subject to appear at the same time. I had had this experience twice before and did not much care to have it a third time.

I was president of the Anthropological Society of Washington that year and would retire in February, 1897. It therefore devolved upon me as "retiring president" to deliver an address, and the date was fixed for February 2. I had not prepared any address and was very busy with many things. I therefore decided to use this paper and try to adapt it to that Society. I gave in the title as *Religion from an Anthropological Point of View.* It was so announced and delivered at that date under the auspices of the Joint Commission of the Scientific Societies of Washington in Builders' Exchange Hall, Thirteenth Street near G. It was a stormy night and the audience was small, but some distinguished persons were present, Mr. George Kennan among others. Mr. Gardiner Hubbard, father-in-law of Mr. Alexander Graham Bell, and very prominent at that time in scientific circles, being president of the Joint Commission, introduced me in a written speech giving considerable of my personal history. I had a "frog in my throat," but managed to get through. The adaptation of the article to the occasion consisted mainly in the following introductory remarks:

> There are some words that I am afraid of, and one of those is the definite article. You will perceive that I have not used it in the title of this address. I do not pretend that I am going to discuss religion from *the only* anthropological standpoint. I shall only approach it from one of many such standpoints. In fact there may be some who will deny that my standpoint is anthropological, and who will insist that it is rather biological, or even cosmological. Upon this point I shall be obliged to leave every one to his own judgment after having heard what I shall have to say.
>
> I imagine also that some may criticise the propriety of selecting religion as the subject of an address to an anthropological society. Probably not a single paper has ever been read before this Society on that subject, and it does not appear among the broad subdivisions of anthropology marked out by our constitution to which I have often pointed as thoroughly comprehensive and encyclopedic. This, too, in my poverty of knowledge of anthropology, I must leave to your judgment and forbearance. But in justification of it I beg to say a word at the outset. This is that religion has always seemed to me to be a typical human fact. I have had considerable to do of late in trying to differentiate the human race from other races, and every year's researches in biology and anthropology increase the difficulties of such an attempt. Fact after fact is constantly coming to light to prove on the one hand, that animal races approach the human race much more closely than had been supposed, and on the other hand, that there are human races and

human attributes that reach down farther and farther till they touch the lower animal world. But there is one thing that, so far as yet discovered, no animal possesses, and it is, singularly enough, this same thing that, so far as yet ascertained, every human race does possess, and that is religion, and so far as I know this is almost the solitary example of which the same can be said. If, therefore, it is improper to treat before an anthropological society a subject which belongs to nothing outside of man and which at the same time relates to all mankind, it would seem difficult to select any subject that would fulfil the requirements.

One further objection may be anticipated. Religion is an old world-wide subject, too broad for treatment in one fleeting hour. My answer is that I propose to confine myself to one single aspect of the subject, viz., to an inquiry into the essential nature of religion.

On February 5, which was Friday, Dr. Alexander Kent, Pastor of the People's Church, which was non-sectarian, called on me and invited me to repeat my address at his church on Sunday; to this I consented. The following announcement was published in all the Washington papers:

So many were prevented by the inclement weather on Tuesday evening from hearing Prof. Lester F. Ward's address on *Religion* that at the request of Dr. Kent and many of the disappointed ones he has consented to repeat it at the People's Church, Typographial Hall, 423 G Street opposite the Pension Office, on Sunday morning, February 7, at 11 A.M. You are cordially invited to attend and to bring your friends.

The audience was large and I was in good voice, and preached my sermon in a very satisfactory manner. I loaned the manuscript to a young man named Gibbs, member of the People's Church, who furnished items to the press, and a fairly good report of it appeared in the *Washington Post* the next morning (February 8).

Dr. Frank Baker, editor of the *American Anthropologist*, the organ of the Anthropological Society of Washington, asked me for my presidential address for publication in that journal. For several reasons I did not want it to appear there, and I put it on the ground that it was not adapted to a strictly anthropological journal. He showed me that the constitution pretty plainly required it, and he seemed to have no discretion. I tried to squirm out of it and finally succeeded by a mutual agreement simply to "let the matter drop indefinitely."

As has been already mentioned (see supra, Vol. IV, No. 214), I gave a number of public lectures in connection with my course of lectures at Chicago in the summer of 1897. For one of these I chose the present subject. This took place on August 31. There I also made some introductory remarks connecting it with sociology for which I had only a few notes. I claimed that sociology covered all subjects, and quoted Phillips Brooks, who, on one occasion when asked what would be the subject of his discourse, replied: "I have but one subject." And I said that one of the Briarean arms of sociology reaches down and clutches at the mysteries of religion.

Dr. Carus continued to delay about the article and said he would publish it as soon as he could make room, but that I was at liberty to recall it if I found another publisher. I wrote to Dr. S. Burns Weston on November 14, and asked him if he thought it would be suitable for the *International Journal of Ethics*. He replied on November 24, that he could not tell without seeing it and would like to have me send it to him. Not having my final draft I hesitated a little, and on December 1, I received a telegram from Weston to send it on at once. I then sent him my rough draft, but wrote to Carus to send the final draft direct to Weston, which he did, but his agent in Chicago sent me at the same time a printed announcement of contributors to future numbers of the *Monist* in which my name occurs for an article on the *Nature of Religion*. Nevertheless the article was speedily put in type, and on December 9, I received both copies of the manuscript and a galley proof. The January number containing it was out on Christmas day, and my reprints arrived the last day of 1897.

The article of course was widely read on both sides of the Atlantic, and I received many appreciative letters. Very few ventured to discuss it, much less to criticise it. Dr. Harris said:

> "Allow me to state that the pamphlet you were so kind as to send me the other day as to the significance of religion seemed to me to be the entrance upon a line in which you would make very important contributions to the science of civilization—even more important if possible than in the *Dynamic Sociology* in which you brought forward such instructive views."

Mr. William H. Ward found in it "an illustration of the tendency which it twice mentions, the tendency to leave out the steps by which results are reached," and adds:

> "I miss the link which connects the fear of the external universe with an institution whose function is to benefit the race by repressing the excursions of the reason. Why such a thought should have embodied itself in such an institution does not appear. It does not appear why such a mechanism is necessary for the realization of this idea. It would not occur to me as an appropriate organ for its expression, and that it would be developed from it in a normal way is not obvious."

Of course the whole subject is much too large for a single article, and I think I have answered Mr. Ward's question in my later writings. But probably nothing would satisfy him. In more modern phrase, religion is a mechanism for social control, and in the absence of government the fear of spiritual beings was the only means to the restraint of wayward tendencies.

On March 5, 1898, Mr. Henry Rutgers Marshall wrote to ask me for a copy of my article, of which he had heard favorably, and I sent it to him. He acknowledged it formally on March 9, and sent me two of his articles, one on *The Religious Instinct*, and the other on *The Function of Religious Expression*, reprinted from *Mind*.

The International Journal of Ethics, Philadelphia, Vol. VIII, No. 2, January, 1898, pp. 169–192.

CAN anything new be said on such a well-worn subject as the nature of religion? If I did not think that there could, I should not attempt it. My point of view may not be new, but I have not met with it. I shall not begin in the usual way by enumerating the various theories of religion, but shall proceed at once to set forth my particular theory.

My thesis is that *religion is a substitute in the rational world for instinct in the subrational world*. But when I say *religion*, I mean, as my title implies, the essential element of religion, which is a very different thing from what is popularly understood by religion. For, while all that is embraced in existing religions of both civilized and uncivilized races is doubtless legitimate and belongs to religion in the sense of having been produced by it, the greater part of it has been superadded to the original nucleus, and this to such an extent that the latter has been lost from view. This process of enveloping the kernel of the religious idea takes place so early that it is difficult to find it even in the most rudimentary religions. Nevertheless it has always been present and active, and to those who understand its nature its workings can be seen at all stages. It is, in fact, intuitively perceived by all, as is evidenced by the almost universal recognition that religion is something good, and this in the face of all the evil that has been done in its name.

The task of unmasking religion is not an easy one. The external garb it wears has come to belong to it. It is organically connected with the primitive form, secreted, as it were, from its tissues, like the shell of a mollusk or the carapace of a tortoise. The alteration that has taken place in religion is not properly a development. It may be compared to the modification that certain plants have undergone at the hands of man. It has been artificially transformed and made to depart from the original type. It always bears the marks of some kind of reasoning, however erroneous.

Although religion is mainly a product of reason, it would be impossible to elucidate its real nature without going much farther back in the history of mind than the origin of the rational faculty, even after conceding all the claims that have been made for the antiquity of that faculty. It may be true, as is asserted of both life and mind, that no point can be indicated at which reason actually began, yet we must recognize that there was a time when it was not. For all the purposes to be subserved in the present discussion it may be said to have begun with man, leaving the question open as to when man himself began. For whatever earlier and feebler states of the rational faculty may have existed, it was only in man that it was sufficiently marked to make religion possible. *Per contra*, no type of human creature has thus far been discovered in which that faculty was not strong enough to generate the religious sentiment.

Anything so universal must surely have a cause in the nature of things. This cause is, of course, psychic, and before it can be understood it is necessary to consider still simpler phenomena. It would be hopeless to attempt an explanation of the rational faculty until the psychic faculty itself was first explained. The reason is a late derivative product, and no clear grasp of its nature can be gained until the nature of mind is first understood. I do not profess to have any unknown secret as to the nature of mind. The biological origin of mind I shall assume to be established, and the principle underlying it need be only briefly set forth. The primordial element of mind is not reason, but *feeling*. A sense of the agreeable and the disagreeable, however feeble, must be assumed as the primary condition to the preservation of the class of organisms in which this sense inheres. This incipient *algedonism* lies at the very foundation of the entire series of psychic phenomena. It is the basis of psychology.

Living things differ from non-living in requiring some means of preservation. They are made up of delicate, colloidal, unstable constituents which are easily destroyed. The organic compounds consist not only of elements of the most complex inorganic compounds, but also of elements of other organic compounds of lower orders, in a rising scale of complexity. Organized bodies are formed out of the highest and most unstable of these latter, such as protein, albumen, and hemoglobin, making substances so complex that their chemical formulas cannot be written. Protoplasm is the primordial organized substance,—the "physical basis of life,"—but it is as much more isomeric and unstable as it is more complex than any of its organic constituents, so that there may be hundreds of different kinds of protoplasm, varying according to a sort of permutation of their organic elements. The very essence of the life principle results from the fact that, in consequence of the relatively immense size of the molecules of protoplasm, due to this recompounding of the already highly complex molecules of organic compounds, the molecular activities of the units are converted into molar activities of the product. This is the only rational explanation I can frame of the phenomena of spontaneous movement, or motility.* Structures, therefore, thus composed, however simple biologically, are exceedingly complex chemically, and this complexity is accompanied by an instability so great that every particle of organized matter is constantly undergoing change by the process called *metabolism*, which is nothing less than the continual falling back of its molecules from the high-tension plane required to constitute life to some lower chemical plane, and the equally unceasing supply of these "waste" elements by a reproductive power inherent in the living mass.

When we consider, therefore, the extreme perishability of organized beings, it is easy to see that for life to exist every possible means must be employed for their protection. The advanced biology of to-day makes it no longer necessary to call in intelligent design to explain these means. In fact, at the same time that the nature of living substance was being revealed by science there was being discovered the natural principle that makes it possible. That principle is natural selection, and we now know that, in conformity with that principle only such organic beings have persisted as have been furnished with some adequate means of self-preservation. There are many such means. Plants possess protective structures of various kinds, and not generally having the power of spontaneous locomotion, which so greatly increases the dangers of most animal forms, they are able to exist without true sensibility to pain. Having for their function to absorb inorganic matter, chiefly in the gaseous or liquid form, they do not require the spur of appetite to impel the pursuit of nourishment. Animals, too, possess numerous other means of protection, such as shells and bony coverings, in addition to sensitiveness. Great fertility performs the same office by constantly filling the places of those that succumb. But in the case of plastic organisms endowed with the power of spontaneous locomotion and compelled to subsist on organic matter, there can be no question that the great and paramount mode of self-preservation is the one pointed out,—viz., susceptibility to pleasure and pain.

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* I first proposed this theory in 1882. See the *American Naturalist*, Vol. XVI, December, 1882, p. 978.

This twofold or algedonic character was necessary, for it was not enough merely to escape the destructive agencies of an impinging and more or less hostile environment. There was an internal destructive agent far more fatal than the external ones,—viz., the very waste of tissue above described as incident to the nature itself of living substance. Metabolism could not go on unless every molecule that fell back to a lower, non-living plane was replaced by another raised to the plane of life by the nutritive powers of the organism. It was not alone necessary to avoid dangers; it was equally necessary to seek supplies. But these expressions, closely examined, are seen in each case to involve an ellipsis. They illustrate the tendency to speak of feeling in terms of function. As a matter of fact it is not danger that is avoided nor nourishment that is sought. The thing shunned is pain, and the thing pursued is pleasure. Safety and nutrition are the indirect or consequential results. The organism knows nothing of these. They are secured by a sort of adjustment of circumstances, a contract, as it were, between Nature and Life. It may also be looked at as a kind of "pre-established harmony" between things wholly dissimilar, by which the one shall always coincide with the other. It is analogous to the harmony between the hands of a watch turned by different wheels at different rates but perfectly adjusted, or to the action of the "cut-off" adjusted to that of the piston in a steam-engine. In short, the principle is that of *adaptation*, which may be conceived in a neuter as well as in an active sense. The adaptations of man are active and *telic*, those of nature are self-adjusting and *genetic*, but the result is the same; and the mechanisms evolved by nature so closely resemble those designed by man that it is only in late years, and now only by special students of nature, that the distinction pointed out has been perceived.

For present purposes even this distinction is immaterial, and if any prefer to regard this great primary bio-psychic adaptation as the work of design, this will not affect the conclusions to be reached. It is only necessary clearly to grasp the truth that, on the one hand, the class of beings under consideration came into possession of a faculty unknown to all other objects, which led them to distinguish the agreeable from the disagreeable and to seek the one and shun the other, and that, on the other hand, those things or acts which possessed the quality of agreeableness were in the main such as supplied the needs of existence, while those which were disagreeable were such as endangered existence. This primordial adaptation, when compared with the multitudinous adaptations familiar in every department of organic life, is found not to differ from these in its essential character. The principle of all organic adaptation is one. That principle is natural selection, and it began to act at the earliest beginnings of life. It may be looked upon as a process of trial and error,—*i. e.*, of trying every conceivable device for rendering vital existence possible and for making it safer and fuller, and the selection of such, and such only, as secured these results. Such experiments usually take the form of structural modifications; but the essential thing is advantageousness, and if this is secured by immaterial qualities these are as legitimate subjects for selection as physical organs. At bottom it comes to the same thing, since every psychic attribute is the result of some modification in the nerve tissues. In the case under consideration the device belonged to this latter class, resulting in an immaterial quality which may be called *feeling*, restricting the term to the intensive form already described, and excluding the indifferent form which later played so great a *rôle* as the basis of the perceptive faculty.* The object may be said to have been to *induce* the creature to perform the acts that would preserve its existence, and no other device is conceivable than the one that was actually adopted. Had no such quality been possible in nature, we seem compelled to assume that animal life would never have been developed.

An adaptation implies two terms. In the present case one of these terms was the action of the organism, while the other was its preservation. Activity may be looked upon as the means and existence as the end. Every device possesses this nature. A telic device, or invention, is the adoption of a means for the attainment of an end foreseen by the designer, and there need be no resemblance between the means and the end. It is the same with genetic devices, if such an expression is admissible. An organ is not at all like its product, any more than is a machine. The product of a machine or mechanism is the foreseen end or purpose. In genetic mechanisms the end is not foreseen, but from the poverty of language to designate genetic products we are compelled to speak of them as the purposes or objects of the mechanisms.

There is, however, a word in biological language which avoids the implication of design. That word is *function*, and the end or purpose of an organ is the function it performs. In the case under consideration, therefore, feeling, which is the means, has for its function the preservation of life. The antithesis, therefore, is constantly between feeling and function.*

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* Cf. "The Psychic Factors of Civilization." Boston, 1893, pp. 16, 21, 39.

While function, in this sense of anything that tends to preserve, continue, or better existence, is thus clearly the end for which feeling was developed, this is true only from the highly general standpoint of the world at large. It is not true from the standpoint of the creature. The end of the creature is feeling itself. Just here is the origin of the great bio-psychic dualism that is indissolubly connected with the entire sentient world. It is as manifest in the highest types of human beings as it is in the amœba. It is the most fundamental antinomy in the world of living things.

The adaptation between feeling and function is so nearly complete that the absolute dissimilarity of the two terms has been universally overlooked. The assumption on the part of biologists of a natural or necessary relation between them is naïve. It is to be classed along with the dictum of "common sense" as to the knowledge the mind possesses of the external world, the supposed identity of mental conceptions with things conceived, of noumena with phenomena. It was this popular misconception, observed by me from the outset, that led me to liken the adaptation of feeling to function to the "pre-established harmony" of Leibnitz.† At the beginning the adaptation must have been virtually perfect, and throughout the lower forms of life it is very complete. Just as the origin itself of sentient life depended upon the closeness of the adaptation, so the continuance and development of this type of life has constantly required the maintenance of this harmony. It is not generally perceived that there is a strong tendency to depart from it. The organism itself has no inherent reason for maintaining it. Its end being feeling, it has no motive to pursue other ends. If, in a changing environment, it is impelled to perform acts that are opposed to its safety or that of its race, it will perform them in harmony with the inexorable law of its nature. The penalty for such acts is extinction, and this has, in fact, been the fate of millions of beings. The organic world has developed under a law which may be called *the elimination of the wayward.*

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* I have been making use of this antithesis regularly in all my works and writings and insisting upon its fundamental nature, and it was not till quite recently that I learned that the principle was not generally understood by non-biological readers. It has always seemed so clear and simple to me that I have never before gone into a logical analysis of it, for fear of being unnecessarily elementary. But I observed that even so clear a thinker as Professor John Dewey stumbled over it (*Psychological Review*, Vol. I., No. 4, July, 1894, p. 403), and the sociologist who has best understood and most appreciated my contributions to that science, Dr. Albion W. Small, in a private letter from which he kindly allows me to quote, said, "I have felt all along that you have connotations in mind which, when explained, would put a different look on your propositions about 'feeling and function,' but I am quite sure you have not expressed them so that we can do justice to your idea. My impression is that you have taken for granted some familiarity with certain technical concepts in biology, and that terms which carry a commonplace meaning with you somehow lose important parts of their content among people not acquainted with that biological usage." In view of all this I feel that I need not apologize for setting forth all the steps in the logical process by which I somewhat intuitively reached this principle seventeen years ago (see *Science*, original series, Vol. I., New York, October 23, 1880, p. 210).

† "Dynamic Sociology." New York, 1883, Vol. I., pp. 469, 602; Vol. II., p. 121.

But nature is full of resources. As structures developed feeling was intensified, and the increase of feeling was attended with increased waywardness and violation of cosmic law. Immediate impulses could no longer be trusted. With higher organization the conditions of existence grew more delicate and precarious. The simple pursuit of food and safety was no longer sufficient. Provision must be made for a longer period of helplessness in the young, and also for future supplies against climatal changes. A curb must be put to the now fully developed *will.* The next great step, necessary to secure these ends, was the origin of *instinct.* Instinct may be looked upon as a device of nature to make the organism desire to perform acts that subserve function, but which would not otherwise be desired. It consists in a readjustment of the nerve plexuses so that acts shall become pleasurable which do not directly satisfy an immediate want. Without these nervous readjustments which lead the creature to act in ways which, from its own standpoint would be illogical, but which in fact make for preservation, it would perpetually pursue pleasures that lead to destruction. But instincts, too, were developed under the law of advantage, or the survival of the fittest, and the obdurate types that refused to adopt them were crowded out of existence.

There is less difference than is generally supposed between simple functions and instincts. Both came into existence in the same way, only the latter represent a higher and more complicated condition. This truth has been clearly expressed by M. Letourneau. He says:

> "Chez les animaux, même chez les animaux supérieurs et chez le premier d'entre eux, l'homme, il existe quantité de coordinations de mouvements, si profondément inscrites dans les centres nerveux, qu'elles sont en même temps inconscientes et indispensables au maintien de la vie; ce ne sont pas des instincts, ce sont des fonctions. Quand la série des actes, enrégistrée dans les centres nerveux, se rapport au genre d'existence de l'animal, lui dicte qu'il en ait ou non conscience, une certaine conduite dans la vie, il en résulte ce que nous appelons des instincts. Mais, au fond, la raison biologique des fonctions et des instincts est la même."*

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* "L'Evolution de la Morale," 2e éd., Paris, 1894, p. 34.

Life is a frail and delicate thing, and in the course of its history it has passed through several ordeals. The first of these was when plastic organisms were created, endowed with locomotion, and dependent for subsistence on organic matter, the condition to the existence of which was feeling, as above described. This was the origin of mind. The second ordeal was when the will* had so strongly asserted itself that existence was put in jeopardy. This was remedied by the development of instincts. Passing over minor ones, we come at last to an ordeal still more severe than any of the previous ones. In the natural upward march of the psychic faculty feeling became at length so potent and its demands so imperative that the direct efforts hitherto employed in its satisfaction no longer sufficed, and a new device was gradually elaborated that should secure the ends of the creature with far greater success. This was the "indirect method of conation,"† and to the affective faculty was now added the perceptive faculty. The intellect was developed as an aid to the will.‡

There is a close analogy, or something much more than an analogy, between the action of the intellectual faculty and that of instinct. Although the method is very different, the purpose is the same, and the general result accomplished by the two faculties is identical,—viz., increased efficiency in securing the satisfaction of desire. We saw that instinct had the effect of making the creature desire to act in ways in which it would not otherwise have acted,—*i. e.*, to act illogically, viewed from the strictly individual standpoint. Now, every indirect action is also, from the same point of view, an illogical action.§ Intellect, too, like instinct, changes the direction of desire. It leads the agent to desire the means instead of the end, otherwise, under the laws of the mechanics of mind, there could be no action.||

But this immense gain, from the standpoint of the individual, almost immediately commenced to threaten the destruction of the race of beings to which this faculty had been committed. It was now so easy to secure the satisfaction of desire, and desires had grown so manifold and so vehement, a larger and larger proportion of them not being adapted to function, many, indeed, being directly opposed to it, that obviously, if, under this new dispensation, everything were allowed to go on without restraint, the race of rational beings must quickly run its course and come to naught. Fortunately, this very perceptive faculty, which was being so freely employed in the interest of feeling regardless of

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* See "Psychic Factors of Civilization," Chapters viii.–xiv.

† "Dynamic Sociology," Vol. II., p. 99.

‡ "Psychic Factors," Part II.

§ See "Dynamic Sociology," Vol. II., p. 380.

|| I have fully explained this principle in a previous article. See *The Monist*, Vol. V., Chicago, January, 1895, pp. 255, 256.

function, was also capable of dimly and intuitively *perceiving* the dangers to which it was leading. Along with the individual mind working thus egoistically for the individual's end, keenly pointing out the ways in which pain could be thwarted and pleasure assured, there was also working, broadly, deeply, and subconsciously, what may be properly called a collective or social mind, solemnly warning against the danger, and authoritatively inhibiting all race-destroying actions. A new device, analogous in many respects to instinct on the lower plane, was gradually developed and perfected *pari passu* with the reason on the higher plane. This device was *religion.*

Religion is, therefore, as my thesis asserts, the substitute among rational beings for instinct among irrational beings. Just as the intellect was developed for the purpose of enabling feeling beings to satisfy by indirect methods desires which they could not satisfy by direct methods, so religion came forward as a more powerful curb to the excesses into which this new egoistic agent would have otherwise plunged its unguided possessors, and for which instinct would have proved wholly inadequate.

It is customary to regard instinct as a substitute for reason, but in so far as there is truth in this view it is in an entirely different sense from that in which the terms are here employed. It is possible that the modifications of nerve structure which result in preservative instincts may receive their initial impulse from a dim consciousness of the necessity for the acts which the instincts impel, so that instincts may be said to rest primarily upon a form of awareness that cannot be generically distinguished from incipient reason. It is also true that many of the acts that are dictated by instinct are the same that would have been dictated by a fully developed reason, had such existed, so that instincts seem to embody a sort of collective wisdom which the individual creatures do not possess. But neither of these assumptions is required by a full comprehension of the law of natural selection, and acts apparently the most intelligent may be accounted for under that law on the part of creatures wholly devoid of intelligence. It is therefore perfectly scientific to look upon instincts as the exclusive products of genetic forces. It is certain from their eminently conservative character that they constitute the necessary curb to the rapidly growing passions of psychically evolving creatures which without them would have inevitably whelmed them in destruction, and which, in obedience to the universal biologic law of the elimination of the wayward, actually did doom to extinction a far greater number than instinct had the power to save.

After the appearance of reason upon the scene, passion not certainly having diminished, but having continued to increase, the new power of gratifying passion only served to multiply the dangers that beset the triumphant legatees of this rich heritage. The very method by which intellect works, far too rapid to give time for the development of instincts, precluded for all future time the employment of this safeguard. A new safeguard must be found commensurate with the forces to be held in check, otherwise the fate of the majority of its predecessors must await the dominant race. Religion, in the primitive and fundamental sense in which the term is here employed, was such a safeguard.

What, then, is religion? If we assume, as I shall do, that it is natural and not supernatural,—*i. e.*, that it grew out of the nature of man, and was not given to him from without, it seems altogether probable that it was mainly a product of reason. It arose simultaneously with the rational faculty, but, as that faculty without some check was essentially destructive, this would have been the case even if reason had had no part in the development of religion; if, for example, religion, like instinct had been wholly due to natural selection. It is not necessary to discuss this point, but personally I am disposed to ascribe it to the joint action of natural selection and reason. These two influences are inversely proportional. The very feeblest forms of reason, while they might cause wide departures from the path of safe activity, could scarcely work out a plan of salvation. At this stage, when the human organism was mainly animal, the survival of the fittest might ultimately result in the development of structural tendencies to the inhibition of destructive actions. Such tendencies would embody the germs of the religious sentiment. They might be called *religious instincts*, but they would differ from normal instincts in merely restraining unsafe activities instead of producing safe ones. In fact, this is the essential characteristic of religion as distinguished from instinct, that it is negative or restrictive, while instinct is positive and constructive.

If the beginnings of religion were genetic, unconscious, and non-rational, this must have constituted a brief and almost hypothetical stage. With the developing faculty which made the satisfaction of desire so easy there must have also grown up an increasing sense of the danger of such unbridled satisfaction. The evil effects of excesses could not have escaped observation. As one after another fell a prey to passion and dropped out of the group, a feeling of the necessity of preventing this grew with the strengthening reason and ultimately took form. This sense, although it must have inhered in the individual mind, was essentially a group sense, and the action in which it resulted was group action. The deep-felt necessity of saving the race inspired the invention and adoption of means to that end. The religious sentiment must be distinguished from its products. The sense of race safety is its very kernel. The primary purpose of religion was at the beginning and always has remained *salvation*. It simply says to the wayward self-seeker, "Thou shalt not," and all the Decalogues have this negative form. But it must enforce its decrees. Even a sense of the penalty for race-destroying acts was powerless to prevent their performance. Some kind of organized system, coercive in its nature, was necessary. The group mind of every race has proved capable of devising such a system. Such are the various religious systems of the world. Whence have they derived their sanction?

There is one fact which all races and peoples, however primitive, and all mankind, however enlightened, have universally recognized. This fact is that there is a power outside themselves which is beyond their control. Rude peoples, living as they always do, in direct contact with nature, are constantly brought into relations with this power and made to feel much more strongly than do civilized races their complete subjection to it. It is this sense of helplessness before the majesty of the environment which, if it is not religion itself, is the foundation upon which all religion is built. It is the *point d'appui* of the group sentiment of safety, that upon which the group reason lays hold to restrain the dangerous activities of the individual reason. Teachers of religion have, I believe, a faint perception of this truth, and at long intervals a more or less definite expression of it may be detected in their utterances, usually, however, so muffled in the blanket of doctrinal language as to be scarcely heard. The following passage is the clearest that has yet come to my notice:

> "If you analyze religion—not my religion, but religion itself—you will find that the essential idea which is its very soul, inheres in the relation which exists between man and the infinite power manifested in the universe above him, no matter what your theory of the universe may be. . . . If you study the lowest type of religion that we are able to discover anywhere on the face of the earth, you will find that the people of that time were thinking of themselves and of some mysterious power not themselves,—some power that held their lives, their destiny, their happiness in its hand; and the object of their religion consisted in getting into right relations with this power. And this getting into right relations with the god has been the one object and aim of religion in every age of the world, and no change of thought can possibly outgrow it. So long as men love, think, and feel, have fears and hopes, so long as the universe stands and is the manifestation as now of this inscrutable power, so long man must be conscious that his life, his health, his sanity, his prosperity, his all, depends upon knowing something about this power, and getting, so far as possible, into right relations with it. Paul may talk about being reconciled to God, and Herbert Spencer may talk about being adjusted to our environment, both are seeking after one and the same thing, which is the knowledge of this power and right relations to it." *

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* From a sermon preached by the Rev. Minot J. Savage, D.D., on the 3d of December, 1893, at Washington, D. C., which has not been published. It was reported in full by Mr. Victor Louis Mason, who has kindly shown me his notes, from which I make the above extract.

Philosophers, too, have occasionally, though rarely, caught a glimpse of this truth, and of such adumbrations I will give one of the least shadowy:

> "D'une part, toutes nos inclinations se rallient sous la seule affection qui puisse les discipliner: d'une autre part, toutes nos conceptions se coordonnent d'après un spectacle independant de nous. En même temps, cette économie extérieure devient la base directe de notre conduite, toujours destinée à la subir dignement ou à la modifier sagement. L'être se trouve ainsi lié, en dedans et au dehors, par l'entière convergence de ses sentiments et de ses pensées vers la puissance supérieure qui détermine ses actes. Alors il y a vraiment ***religion***, c'est à dire unité complète, tous les moteurs internes étant coordonnés entre eux, et leur ensemble librement soumis à la fatalité extérieure. . . . Telle est l'issue finale du grand dualism positif entre l'organisme et le milieu, ou plutôt entre l'homme et le monde, ou, mieux encore, entre l'humanité et la terre."*

It must be remembered that with primitive man the unknown was vast. He was absolutely ignorant of the stupendous forces of nature playing about him. It is no part of my purpose to trace the growth of religions out of religion. This has been tried many times, but the history has never seemed to me to go back to religion itself. Dr. Edward B. Tylor, than whom no one has more ably treated this class of subjects, has reviewed all known primitive religions, and he arrived at what he aptly styled the "minimum definition" of religion,—viz., "belief in spiritual beings."† Doubtless from the standpoint of the ethnographer this definition is the simplest that could be formulated. It satisfies all the requirements of a natural history of man; but from the standpoint of general or cosmical philosophy it falls far short of a principle that can explain the phenomena of religion. Belief in spiritual beings is not religion itself, but a consequence of religion. It grew out of the fact upon which I have insisted, that the race was in danger from unknown and uncontrollable agencies, and the same faculty that perceived this also sought to explain it. Spirits, ghosts, fetishes, gods,—these were only explanations, deductions, conclusions, products of the dread of the unknown. *Primos in orbe deos fecit timor.* Mythology is philosophy. Theology and science start from the same stake.

It remains to point out how religion took the place of instinct on this higher rational plane, and served effectually to check the evils that reason had so greatly multiplied. Although the *sense of dependence* above described is that which made religious systems possible, it could no more manifest its saving virtues without an organ than a sensory system can act without a motor system. Every human thought, in order to accomplish any purpose, must develop a mechanism through which it can work. Every human institution is the product of a thought. The thought went before and produced the institution. The institution is the mechanical embodiment of the thought. It was so with religious institutions. The religious idea, as I have endeavored to sketch it, worked on the group, on society (for association was taking place at the same time), and in the normal way developed the appropriate organ for its expression, the necessary mechanism for its realization. Religion as an institution is a different thing from religion as an idea. The institution arose like all other human institutions, as a product of the social forces brought into equilibrium for the storage and economical expenditure of social energy.* As such it was effective in accomplishing its purpose,—*i.e.*, in performing its function.

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* Auguste Comte, "Politique Positive," Vol. II., p. 18.
† "Primitive Culture." London, 1871, Vol. I., p. 383.

In the great dualism of life, religion is the champion of function against feeling, of the race against the individual. It is race reason working for function against individual reason working for feeling. It represents the primordial conservatism of mankind. There has never been a time when the desires of men did not impel them to perform acts destructive to the race. The mandate of individual reason is to obtain satisfaction. Interest is its only guide, the interest of the individual. Now, it constantly happens that acts which are in the interest of the individual are against that of the race. A man may pursue egoistic ends successfully during life by acts that do not react upon himself or tend to do so, and yet his whole course may be inimical to society at large. Most self-seeking is of this nature. The fine maxims to the contrary, and the stories in the Sunday-school books, are merely myths invented by religion to deter from self-seeking. So long as any continue to believe them they have this effect by inspiring fear of consequences or hope of reward for good deeds. But the truly wise no longer believe that "the way of transgressors is hard," or that "the righteous are never forsaken." The same book that teaches these things also teaches that "he that seeketh findeth," and the persistent seeker after his personal advantage usually succeeds in finding it. Says John Stuart Mill:

> "The very idea of distributive justice, or of any proportionality between success and merit, or between success and exertion, is in the present state of society so manifestly chimerical as to be relegated to the regions of romance. . . . If persons are helped in their worldly career by their virtues, so are they, and perhaps quite as often, by their vices: by servility and sycophancy, by hard-hearted and close-fisted selfishness, by the permitted lies and tricks of trade, by gambling speculations, not seldom by downright knavery."†

Not that there are not vices which react upon the individual and ruin him, but these are the results of irrational impulses.

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* I have developed this principle in an article entitled "The Mechanics of Society." See the *American Journal of Sociology* for September, 1896, Vol. II., No. 2, pp. 234–254.

† *Fortnightly Review*, Vol. XXXI. (New Series, Vol. XXV.), 1879, p. 226.

The class of actions here under consideration is that of rational calculation aimed at self-satisfaction. Such actions are opposed to the safety of society because the gain to self is chiefly at the expense of others, and the number injured is far greater than the number benefited. The algebraic sum is a loss of social efficiency and a menace to the race. All forms of exploitation of the many by the few come under this head. The evil is twofold. It affects the existing group and it affects the future. It need not at all affect the individual either at the time or at any other time. Such also are all destructive actions that waste the accumulations of time and labor.

Another important class, and the one that religion earliest grappled with and still contends against, results from the powerful reproductive passion. It was possible for primitive peoples to pursue this form of satisfaction egoistically, to lead unregulated, promiscuous lives, with no concern for the functional consequences, and the result would be that while no evils might attend the individuals so acting, while the male sex at least might have considerable to gain and little to lose, the offspring would be neglected and would perish and the race would be, if not destroyed, at least so reduced in strength as to become a prey to other races that had learned to regulate this class of conduct. Religion has been the chief agent in the organization of marriage systems and in establishing the family relation upon which society so largely rests.

But it is needless to multiply examples. Suffice it to say that the mission of religion was to restrain the will. It represented the race and the future; it denied the claims of feeling; it demanded sacrifice. It may be called the *social instinct*. But there are two things commonly claimed for it that form no part of its purpose. These are *altruism* and *progress*. Although the natural enemy of egoism because race-destructive, and although perpetually enjoining group action and social action, religion in its essence has nothing to do with sympathy, or any other kind of 'pathy. It deals with function, not feeling, and simply serves Dame Nature in her great cosmic scheme of preserving, perpetuating, and increasing life. If to these can be added the perfectionment of living beings, this is only because such perfectionment is a means to the supreme end. It has no reference to the deepening or heightening of the quality of sentiment. Anything in existing religions that seems to contradict this statement is something superadded to religion itself —some late graft upon the original stock—and belongs to a modern period.

As regards progress, the case is still clearer, and here we encounter a singular paradox. Religion may be regarded as the guardian of order, but not for the purpose of securing progress. The progress of the world has consisted entirely in the degree to which the claims of feeling have commanded recognition. What is meant by progress when speaking of man is never the mere increase in the number of human beings. The attempt thus to define it was Mr. Kidd's fatal mistake.* In so far as it ever refers to the perfection of the human type progress has in view man's capacity to enjoy. Social progress consists in the better realization of the power to satisfy want. Æsthetic progress is the increase and refinement of tastes and the increased opportunity to gratify them. All forms of progress are so many victories of the will over nature, of feeling over function. It is in the nature of religion to resist these tendencies. They are centrifugal, and therefore dangerous. Religion is the force of social gravitation that holds the social world in its orbit. The primary analogy is this cosmic one which likens religion to the gravitant and reason to the radiant forces of the universe. Upon this I need not dwell. But there is a biologic analogy which comes much nearer home, and which it is important to note. This likens religion to heredity and reason to variation in the organic world. The former preserves the type while the latter modifies it; the one tends to fix the species, the other to transform it. Biologists are now familiar with this great antithesis which inheres in the entire history of life. Even Goethe gave expression to it, and in the following form:

> "Die Idee der Metamorphose ist gleich der *vis centrifuga* und würde sich ins Unendliche verlieren, wäre ihr nicht ein Gegengewicht zugeben: ich meine den Specificationstrieb, das zähe Beharrlichkeitsvermögen dessen, was einmal zur Wirklichkeit gekommen, eine *vis centripeta*, welcher in ihrem tiefsten Grunde keine Aeusserlichkeit etwas anhaben kann."†

Von Baer arrived at the same truth independently, and the transformist school of biologists have shown that adaptation and modification are the products of the law of variation held in check by the power of heredity. Human progress has followed a precisely similar course, and has been the product of the law of reason held in check by the power of religion. When, therefore, we say that religion is non-progressive, we mean nothing more than when we say that heredity is non-progressive. But for the law of variation there would certainly be no organic development, and if religion worked alone there could be no human progress. But, on the other hand, it could be said that without the hereditary check the organic world would be whirled out of existence by the unrestrained tendency to vary, and, similarly, without the religious check the human race would have been borne to destruction by the extravagant vagaries of the unbridled reason. It is not progress that either heredity or religion opposes, but change, and progress in both the organic and social realms is a compromise between centripetal and centrifugal forces. Cosmic, organic, and superorganic evolution takes place according to one principle,—the principle of organization, which consists in the natural storage and economic expenditure of energy through progressively higher products of these harmoniously interacting antagonistic forces.*

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* "Social Evolution," by Benjamin Kidd. London and New York, 1894.
† Goethe, "Metamorphose der Thiere."

Another point of view from which this fundamental antithesis may be contemplated is that of economics. That science deals exclusively with *utility*. Religion may be said to be diametrically opposed to it in that it deals exclusively with *necessity*.† This explains its superior sanction. The primary requisite is to live. If existence is lost all is lost. It is useless to discuss degrees of enjoyment until life is first secure. As a matter of fact, man has been thus far chiefly engaged in a struggle for existence. It is only in the later stages of social development, and among the most favored classes of society, that there has been any energy left to expend in efforts to be happy. The mass is everywhere exhausting all its energies in the effort to live. This is largely due to social and moral restraints, to exploitation by the egoistic reason of the specially endowed or favorably circumstanced. Formerly it was chiefly oppression,—*i. e.*, the exploitation of the governing class. The priesthoods that have embodied the germ of religion have contributed their share, and the essential conservatism of religion has usually allied it with the "powers that be." Man's struggles for liberty, of which human history is chiefly the record, have consisted simply in so many assertions of the claims of feeling against function, so many demands of utility upon necessity. Human progress has consisted in the reluctant concession of such of these claims and demands as did not threaten existence.

Throughout western Europe and America this struggle is chiefly over. The ruling class is at the feet of the Demos and the power of the priesthood is broken. But a new kind of oppression has taken the place of the old. A new set of rulers has come forward. The owners of the means of subsistence now dictate to the masses. The struggle for existence goes on as before, and, as before, it is the egoistic reason, coupled with the accident of position and backed by the whole body of proprietary law, that is able to exploit the mass not favored by these conditions. The "labor question" of to-day is a new assertion of the claims of feeling against function, a renewed demand of utility upon necessity. It is essentially utilitarian, and economists are just beginning to understand that their science is dynamic, and that "consumption," *i. e.*, enjoyment, *i. e.*, feeling, constitutes its true basis. To all of which religion, from its very nature, is opposed.

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* I have worked out these principles in several articles. See, especially, "Cosmic and Organic Evolution," *Popular Science Monthly*, Vol. XI., October, 1877, pp. 672–682; "The Natural Storage of Energy," *The Monist*, Vol. V., January, 1895, pp. 247–263; "The Mechanics of Society," *American Journal of Sociology*, Vol. II., September, 1896, pp. 234–254.

† I use these terms in the sense in which they were defined by me in the *American Journal of Sociology* for March, 1896, Vol. I., p. 627.

Here, however, an important qualification is to be noted. The mind naturally tends to leave out the steps by which it has arrived at results. This tendency was mentioned in the early part of this paper, where it was shown that it is not nourishment that the animal seeks, but pleasure; not death or destruction that it fears or flies from, but pain. Now, just as we habitually speak of feeling in terms of function and neglect the important step that lies between them, so in speaking of religion as opposed to pleasure we employ a similar ellipsis. It is not pleasure that religion opposes, but danger; and pleasure is so often dangerous, especially in the earlier stages of existence,—in the state that Dr. Patten has so expressively denominated a "pain economy,"* —that the two are ultimately confounded. This explains the next point to be considered.

It belongs to the true nature of religion constantly to yield to the demands of reason and of feeling. It is no reproach to it that it has so yielded. Its opposition, as we have seen, is not based on any hostility to pleasure *per se*. It is only as a source of danger that it attacks it. Its mission is not to destroy, but to conserve. It is not pleasure itself that is bad, but the acts it produces. Selfish greed is execrable, but the fruits of honest toil are noble. Promiscuity is hateful, but wedlock is sacred. Just as gravitation must compromise with radiation to produce an orb of space, just as heredity must compromise with variation to develop an organic being, just as function must compromise with feeling to make an animal world possible, just as instinct must compromise with will to secure the highest animal development, so, on the same general principle, and in strict continuation of the same grand series of cosmic steps, must religion yield to the demands of reason wherever they are safe before anything worth conserving can be produced by man.

It is true that many of the late derivative products that call themselves religions do combat pleasure *per se*, but, as I have shown, these are artificially distorted forms, and such doctrines are excrescences that have grown out of the normal religious trunk. They are so far removed from the region of healthy circulation that the very ground of the doctrines is lost sight of, and the means that true religion employs are mistaken for its end. All forms of religious austerity, of self-abnegation, asceticism, puritanism, etc., are abnormal growths of this kind, and, like certain anatomical organs that once had a purpose, but are no longer needed, they have become pathologic. This tendency to survival is universal. The persistence of the obsolete is a principle of the organic and superorganic worlds alike. The anchoret hates pleasure for the same reason that the miser loves money.

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* See his "Theory of Social Forces." Supplement to the *Annals of the American Academy of Political and Social Science* for January, 1896.

It follows that the recognition of the claims of reason to the full satisfaction of feeling must be yielded by religion as fast as adequate guaranties are furnished that everything useful thus far created at so great expense shall be conserved; and the most ardent votary of progress ought to admit that without such guaranty it should not be yielded. The question, therefore, naturally arises, Will religion ever cease to furnish a necessary check to the extravagances of reason and of passion? We saw that religion has its sanction in the mystery of the universe, that it is the vastness of the unknown that furnishes it its hold upon mankind. Will that chasm ever be closed? Shall science, brushing aside one after another the cobwebs that obscure the vaults of nature, at last so completely lay bare her secrets that no hidden terrors will remain? Are science and religion, which, as we saw, started together, but which have so widely diverged in their course, destined to converge and ultimately to coalesce? Will science finally swallow up religion, assume its functions, and stand wholly in its stead? We can only answer these questions by noting the history of science thus far, by considering its long series of conquests and the steady but orderly recoil of religion as the guaranties of safety were one after another placed in its hands. Noting this, and comparing the condition of the vanguard of civilization with that of its dark beginning, it seems impossible to doubt, not that religion is ever to disappear, but that it will ultimately put all confidence in reason and cease to raise its voice against the realization on the part of man of the fullest capabilities of his nature.

Numerous objections may of course be raised by the captious to the general theory above outlined. I shall only attempt to anticipate one of these. Some, I fancy, will say that it is not religion that I am describing, but ethics. I think I can show that this is not the case. No two things have been more hopelessly confused than ethics and religion. By way of clearing up this confusion, it should be stated at the outset that ethics is wholly concerned with feeling,* while, as I have shown in this article, religion is wholly concerned with function. Pleasure and pain furnish the only basis for a moral quality, while religion has nothing to do with pleasure and pain, but is solely devoted to the maintenance of life. It is exclusively a cosmical agent. In so far, then, as moral philosophers have occupied themselves with this latter problem they have been dealing with religion, and whenever religious teachers talk of happiness and misery they are encroaching upon the domain of ethics. Superficial observers may say that religion chiefly deals with happiness and misery—in another world. Two of the great religions of the world do, in fact, deal mainly with this aspect, but they do not thereby refute the above statements. The doctrine of immortality is one of those late grafts upon the primary stock of religion to which reference has been made. Moreover, it is one of the chief concessions that religion has been compelled to make to the demands of reason for the recognition of the claims of feeling, a concession which was found to be altogether safe. These demands had become irresistible in the days of Plato, and the creation of a future state in which feeling should be everything was one of the most sweeping of the whole series of compromises between religion and reason. By it religion saved itself from being completely set aside by the rational will of a determined race, while reason accepted it in the face of its unthinkable assumption of a beginning without an end, an eternity exclusively *a parte post*.

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* I have furnished a demonstration of this statement in another paper. See INTERNATIONAL JOURNAL OF ETHICS for July, 1896, Vol. VI., pp. 443, 444.

It has been argued that Western peoples are proved by this to be less logical than the Orientals, with their philosophically superior doctrines of transmigration and nirvana, but this is probably not the case, and the explanation is to be sought in the greater power of the egoistic will of the Western world, which rudely thrusts aside all subtle points of logic and makes them wholly subordinate to considerations of interest. The doctrine of immortality, like all the other dogmas of modern faiths, has no connection with the essentials of religion, although it was a necessary phase in the history of the vicissitudes that religion has been compelled to pass through.

I will conclude with a personal remark. Much has been said on the subject of a reconciliation of science and religion. I have not been one of the reconcilers; but if the definition of religion which I have here formulated is correct, it embodies a reconciliation far more complete than any that have been hitherto proposed. If it seems to differ from the view of religion that I have formerly expressed, it is because, in common with most others, I have heretofore taken the actual condition of religion as representing its real nature. I am far from saying that there is anything essentially incorrect in this, and it may well be that what I have here called religion ought to go by some other name. If it is not religion, it is without a name. The name is certainly a matter of little moment. If I have described something that has hitherto escaped attention, I am quite willing another shall give it a name. But that I have in the present essay outlined a chapter still to be written in the history of life, of mind, and of man, I shall remain convinced until clearly shown to be in error.

December, 1897—Ætat. 56.

445. ["The Increase and Diffusion of Knowledge among Men"]

History.—Written September 2, 1897. One of the public lectures that I delivered in Chicago in 1897 was my old one with the above title, which is the celebrated clause in the will of James Smithson, stating the purpose for which he donated his fortune to the United States. It was written from January 25, to February 13, as one of the Saturday Lectures delivered in the Smithsonian Institution. It has never been published. After I had delivered this lecture in Chicago Mrs. John F. Ottley of Atlanta asked me to write something in her "Thought-book." She had manifested a special interest in this lecture, and I wrote the concluding passage of it in her book, to which I added a sentence containing its main argument. At a meeting of the General Federation of Women's Clubs, held in Nashville in the fall of that year, Mrs. Ottley delivered an address on *University Extension in its Relation to Women's Clubs*, in which she took occasion to quote what I had written in her "Thought-book." Her address was published in a leading educational journal, and thus that little "Glimpse" managed to get into print.

The Northwestern Monthly, Lincoln, Nebraska, Vol. VIII, No. 5, December, 1897, p. 334.

Of all the panaceas that have been so freely offered for the perfectionment of the social state, there is none that reaches back so far, or down so deep, or out so broad, as that of the increase and diffusion of knowledge among men. But the increase of knowledge—the discovery of wholly new truths—is attended with such a charm that it may be trusted to take care of itself. *The great social duty, therefore, is the universal diffusion of all the most important knowledge now extant in the world.*

December (?), 1897—Ætat. 56.

446. A New Species of Eucalyptus from the Dakota Group of Southwestern Kansas

History.—Written December 3–4, 1897. I had given the specimens to my draftsman two days before, and wrote the description with both the fossil and the drawing before me. The exact locality at which I collected the leaf on October 3, was on Chatman Creek five or six miles north of Ashland, Kansas. The specimens lay on the surface of the ground. I offered the paper to Dr. Knowlton for the *Plant World*, but he declined it, so I sent it to Dr. Hollick who published it in the *Torrey Bulletin*. The greater part of it, including the figures, was copied from the *Torrey Bulletin* into the *Plant World* for February, 1898.

Bulletin of the Torrey Botanical Club, New York, Vol. XXIV, No. 12, December, 1897, pp. 576–577. The Plant World, Binghamton, N. Y., Vol. I, No. 5, February, 1898, pp. 75–76.

January (?), 1898—Ætat. 56.

447. Bibliography for the year ending June 30, 1895

History.—My bibliography has always been regularly kept up in this form, new titles being added as the works or articles appeared. When called upon for that of any particular year or period I would make a copy, or have it made, from my list and send it in to be incorporated with those of other members of the staff.

Annual Report of the Board of Regents of the Smithsonian Institution for the year ending June 30, 1895. Report of the U. S. National Museum. Washington, 1897, pp. 227–228.

WARD, LESTER F. Recent discoveries of cycadean trunks in the Potomac formation of Maryland.

Bull. Torrey Botan. Club, XXI, No. 7, July 20, 1894, pp. 291–294. Gives an account of a collection of cycadean trunks made by Mr. Arthur Bibbins in Maryland, and the peculiar manner in which it was made, the specimens being all found in the possession of private individuals; also of efforts made to determine the geological horizon at which they originally occurred.

—— [Note on Professor Jenney's collection of fossil plants from the Lower Cretaceous of the Black Hills.]

Science (New Series), I, New York, Feb. 1, 1895, pp. 137–138.

—— The Mesozoic flora of Portugal compared with that of the United States.

Science (New Series), I, New York, Mar. 29, 1895, pp. 337–346. A review of the literature of the Mesozoic flora of Portugal, and especially of the recent elaborate memoir of the Marquis Saporta and M. Paul Choffat, with special indication of the analogies, both stratigraphical and paleontological, to the older Mesozoic and the Potomac formation of the United States.

—— Remarks on the genus *Caulinites*, Brongn., with exhibition of specimens (rhizomes of *Tripsacum dactyloides*).

Science (New Series), I, New York, June 28, 1895, pp. 725–726. Abstract of a paper read before the Biological Society of Washington, June 1, 1895. These rhizomes very closely resemble *C. parisiensis* (Desmarest) Brongn.

—— [Fossil plants.]

Johnson's Universal Cyclopedia (New edition), VI, New York, 1895, pp. 639–645.
A somewhat complete account of the past history and present state of the science of paleobotany.

—— Report on the Department of Fossil Plants in the U. S. National Museum, 1892.

Rep. Smithsonian Inst. (U. S. Nat. Mus.), 1892 (1893), pp. 185–190.

January, 1898—Ætat. 56.

448. Psoralea tenuiflora as a Tumble-weed

History.—Written December 3, 1897. The drawing was made by my draftsman.

The Plant World, Binghamton, N. Y., Vol. I, No. 4, January, 1898, p. 59.

January 11, 1898—Ætat. 56.

449. Literary Note [Outlines of Sociology]

History.—Written November 28, 1897 (see supra, No. 441).

LITERARY NOTE

Outlines of Sociology, by Lester F. Ward, LL.D., lecturer on sociology at Columbian University, Washington, D. C., is the title of a book announced by The Macmillan Company. This work appeared in serial form as "Contributions to Social Philosophy" in the *American Journal of Sociology* from July, 1895, to May, 1897, and is treated from the advanced standpoint of active discussion on both sides of the Atlantic during recent years.

The object of the work is to furnish a clear idea of the science of sociology, not by means of verbal definitions, but by clearing the ground and removing the entanglements with other cognate or ancillary sciences, and by a succinct presentation of its principles as distinguished from its elements.

The plan is, therefore, two-fold: First, to bound the science and mark it off from other sciences which surround it and impinge upon it, showing what these sciences are and how they are related to it; secondly, to lay out the science of sociology itself in a definite scheme of laws and principles susceptible of true scientific treatment.

The first of these tasks, which the author calls "Social Philosophy," and to which he devotes the first six chapters, or just half of the book, is the subject of Part I, while the second, which he distinguishes as "Social Science," and which occupies the latter half of the book, constitutes Part II. The work is, therefore, devoted to the distinguishing of social philosophy from social science, and to the showing of (1) the relations of sociology to other sciences, and (2) its essential nature as a science.

January 13, 1898—Ætat. 56.

450. [Dr. Schenk's Method of Controlling Sex]

History.—Interview with Mr. L. D. Sale, Reporter for the New York *World*, January 12, 1898. It is printed substantially as I dictated it, but does not give all that I said.

The World, New York, Vol. XXXVIII, No. 13295, Jan. 13, 1898, p. 3.

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January 17, 1898—Ætat. 56.

451. Utilitarian Economics

History.—Written February 14, to March 1, 1896. At the close of the session of the American Economic Association in the afternoon of December 30, 1895, at Indianapolis, Dr. Patten asked me to work up the subject of my remarks on his paper (see supra, Vol. V, No. 396) into an article for the *Annals* of the American Academy of Political and Social Science. Of course he knew that I had read his *Theory of Social Forces* and was familiar with his doctrine of "pain and pleasure economy." He had sent me that memoir early in the month, and I was preparing my review of it (see supra, Vol. V, No. 403). I stated in that review that I hoped before long to treat that subject at greater length. The present article is a part, but not all of that fuller treatment (see infra, No. 454).

The outline was completed on February 23, 1896, but in rewriting it, as I then proceeded to do, I modified it extensively. As soon as the final draft was completed I sent it to Dr. Patten and also wrote to Dr. Falkner, Editor of the *Annals* with regard to it. As it was practically ordered, I thought they might pay me at least a nominal price for it. But there was the hitch, and after considerable correspondence, and many expressions of regret on their part, I finally withdrew it on May 24, though through some trouble with the mails it did not arrive till June 8. I then revised it again and made a number of changes, and sent it, on June 10, to Dr. Dunning, offering it to the *Political Science Quarterly*.

While the *Annals* folks had it Dr. Small somehow got wind of it, and in the same letter of March 24, in which he invited me to give the summer courses in sociology during his absence the next year, he expressed a strong desire to print it in the *American Journal of Sociology*. He wanted to interrupt the series of articles on *Social Philosophy*, which began with the first number, and of which one had appeared in each number ever since, by interpolating this article which did not belong to the series. The reason he gave, as expressed in his own words, was that he had found that

> "a good many people think you have departed from your first faith and relapsed into the feeling that all men can do is to wait for the mills of the gods to grind along at their slow pace, with no acceleration from human coöperation."

The fact is that I was lying back against the time when I should be "good and ready" to write my *Applied Sociology*, which, even so early, was rumbling in my brain.

Then again, I did not much relish the idea of interrupting that series, and I thought that the later papers on *The Purpose of Sociology* and *Collective Telesis* would appease the ones who feared that I was backsliding.

After a long delay Dr. Dunning finally returned my *Utilitarian Economics* on November 19, apologizing for keeping it so long, and saying:

> "We are very reluctant to let so valuable and suggestive a piece of work go elsewhere; but it is a little farther over in the biological direction than we are disposed to claim as our field, and we are hard pressed for the matter that is distinctly in our line."

In December I worked all the changes I had made in the final draft into the original one, and commenced using the latter along with another manuscript on a similar subject, as the basis for a paper that I proposed to present at the next Congress of the Institut International de Sociologie. This required it to be put into French, which I proceeded to do (see infra, No. 454). I also worked the greater part of this article into a lecture that I delivered at Greenacre on July 19, 1897, entitled: *The Real Moral Evolution*, and which I delivered again with considerable modifications at the University of Chicago on August 25, where I called it *Pain and Pleasure Economy*.

The series of articles on *Social Philosophy* was completed in the May number of the *American Journal of Sociology*, and there was no longer any reason for not allowing this paper to appear there, as Dr. Small had desired, and after my return from the field in October, although Dr. Small was still abroad, I wrote to Professor Henderson on the subject. He replied on November 4, asking me to send him the article, which I did immediately.

This article is an illustration of the general truth that the more thought there is in an article the more difficult it is to get it published (see supra, Vol. V, No. 373). Of course the principle is broader, and it is equally true that the more worthless a literary production the more it will bring in the market and the more widely it will be read, until we get to the most trashy novels which run through hundreds of editions.

The American Journal of Sociology, Chicago, Vol. III, No. 4, January, 1898, pp. 520–536.

January 29, 1898—Ætat. 56.

452. Outlines of Sociology

History.—The history of this book is chiefly that of its twelve chapters, and that has already been written (see supra, Vol. V, Nos. 387, 390, 392, 399, 402, 406, 413, 418, 422, 428, 430, and 434). The rest relates to the manner in which they were gotten together. But this was very simple, for the twelve articles constitute the twelve chapters, and are arranged in the same order and for the most part have the same titles. All that is new is the dedication, the preface, the index, and part of one foot-note (see supra, Vol. V, No. 434). The last of the articles appeared on May 12, 1897. I had ordered covered reprints of all the articles, and I put a set together and used them as a book in my work. It naturally occurred to me that if properly bound together they would make a small convenient book, and I carried out this thought.

On November 1, 1897, I arranged a set of these reprints in proper form for printing and proceeded to write the preface. On November 4, it occurred to me that there was a fundamental difference between the first six and the last six papers, and I divided the book into the two parts, *Social Philosophy* and *Social Science*, and pointed out the distinction in the preface, which I finished on November 5. On November 8, I sent the book thus arranged to the Macmillan Company and asked them to publish it. I had prepared the title page exactly as it finally appeared, calling the book *Outlines of Sociology*, putting in special title pages for the parts, and numbering the pages of the whole consecutively from 1 to 194. On November 17, I received notice of its acceptance, and on the 20th the articles of agreement were signed. On November 21, I sent on the dedication of the book to Dr. Small.

On November 25, I put together a set of the reprints and sent them to Miss Boshniak in Moscow, who had translated my *Psychic Factors of Civilization* into Russian (see supra, Vol. V, No. 349), wrote her a letter telling her this was to be published in book form, asking her if she could translate it, and suggesting that she could gain time by beginning on the reprints until the book appeared. The book of course was sent her as soon as it appeared. When I returned from the field on June 28, 1901, I found in my mail a copy of the Russian translation. On the title page were written the words: "Better late than never. A Russian proverb." I read it all through with the aid of my Russian dictionary. It bears date Moscow, 1901.

Sample pages of the book came on November 25, and the work of composition proceeded at once and rapidly. Proofs began to come on December 6, and the last plate proof of the text reached me the last day of 1897. As soon as final proofs began to come, viz., December 20, I commenced preparing the index, and it was completed the day after Christmas. It was all over by January 4, 1898; the book was put on the press at once, and an advance copy was received January 29. Additional copies arrived on February 9.

The foot-note on pp. 180–182 was much expanded from the form used in the article on *The Mechanics of Society* (see supra, Vol. V, No. 418), as the result of further researches and correspondence with Dr. Huntington and Dr. Seymour. I have long letters from both of them on the subject, going over the ground much more fully, but I do not see that they add anything to what I finally put into the foot-note. Two unfortunate errors occurred in this foot-note on p. 181. The name Athenaeus was printed Ath*a*naeus, and the letters *so* were dropped out of the word *Deipnosophistae*. The corrections were sent on as soon as the errors were detected, and they are made in the second impression of the work, which appeared in 1899. The mistakes were both my own and not the printers', as they occur in my manuscript of this foot-note which is in my handwriting and is pasted to the printed copy of the text. Another mistake due to my carelessness, and which has not yet been corrected, occurs on p. 77, where Archbishop Whately is referred to as "Bishop Whately."

The book was distributed mainly by the publishers without any special effort on my part, and I received very few acknowledgments. Most of my correspondents had already received all the reprints constituting the chapters. Many reviews appeared, some of which were able and important. Such were the ones by Professor W. B. Owen in the New York *Times* for June 11, 1898; the one in *Nature* for September 22, 1898, signed by A. E. Taylor; and the one in *Science* for December 16, 1898, by Mr. Gustavo Tosti. Of foreign reviews I will mention only that by Professor Enrico Morselli in the *Rivista Mensile di Filosofia Scientifica* (Rome) for December, 1899, and the longer one of Professor Alessandro Groppali in the *Rivista Mensile di Filosofia, Pedagogia e Scienze affini* of Bologna, Anno I, Vol. II, No. 2, Bologna, 1900, and embodied in a pamphlet entitled *Rassegna di Sociologia e Scienze affini.* Bologna, 1900, pp. 3–6. I had corresponded with both these writers.

In a letter from Professor Adolfo Posada, dated Madrid, March 2, 1905, he informed me that a publisher had offered to publish a Spanish translation of this work, and asked me for my authorization, which of course I promptly gave. On May 4, 1906, soon after my arrival in Paris, I received another letter from Professor Posada, dated April 14, which had been forwarded to me from America, in which he stated that the translation was completed, but he was in doubt as to a proper Spanish title for the book. He mentioned several, one of which was: *Compendio de Sociologia.* This I much preferred to any of the others. In fact, I had once thought of calling the book: *A Compendium of Sociology.* On June 12, I received from him a proof of the title page in which this title was adopted. On November 16, after my return to Washington, I received six copies of the Spanish translation. It is neatly gotten up and is a thorough piece of work. I understand that it was translated wholly by Professor Posada himself. I read it all through carefully and compared the English text, and I noted all the real or supposed typographical errors, of which I sent Professor Posada a list. None of them were serious, though some were a little amusing. It is a thorough piece of work, and the wonder is that such a prolific author could find time to do it. The *Heraldo de Madrid* of November 5, 1906, contains a notice of the Spanish translation, with a reproduction of the photograph of myself that I had sent to Posada, at the request of the publisher. The book begins with a "Prologo del traductor" which is highly flattering to the author.

March, 1898—Ætat. 56.

453. Review of The Social Mind and Education. By George Edgar Vincent. Price $1.25. Pp. ix, 156. New York: The Macmillan Company, 1897

History.—Written January 10–13, 1898. This book was sent me by the publishers on December 22, 1897, and I had read it through by December 29. I then wrote Professor Vincent a letter, proposing to review it, and received a reply dated January 1, 1898, saying he hoped I would review it and suggesting that it would look better for the review to appear somewhere else than in the *American Journal of Sociology*, and mentioning the *Annals*. So when I had written the review I sent it there and it was accepted.

Annals of the American Academy of Political and Social Science, Philadelphia, Vol. XI, No. 2 [45], March, 1898, pp. 264–267 [128–131]; Reprint, pp. 1–4.

April, 1898—Ætat. 56.

454. L'Économie de la Douleur et l'Économie du Plaisir

History.—Written December 25, 1896, to April 22, 1897. This article consisted mainly of the French version of my two articles entitled *The Subjective Trend of Modern Philosophy*, and *Utilitarian Economics*. The history of the second of these papers will be found under that title (see supra, No. 451). The other was my eighth Hartford lecture and is mentioned under the history of the first of those lectures (see supra, Vol. V, No. 387). It was never published under that title, and the matter of it has been distributed to other places. About all of it went into this French paper, and most of what was not included in the *Utilitarian Economics* was embodied in my *Applied Sociology* (pp. 29–36). I still possess the unpublished manuscript of that lecture. It was written July 11–21, 1895, and delivered at Hartford on November 5, 1895.

On August 31, 1896, I was elected a member of the Institut International de Sociologie. I received my notification on September 10. Dr. Worms asked me to participate in subsequent Congresses of the Institut, and I decided to do so. I chose as my subject for the next Congress (1897) the one that was uppermost in my mind at that time, viz., Dr. Patten's idea of pain and pleasure economy, and on November 29, I wrote to Dr. Worms proposing to contribute a paper on that subject. On December 13, he wrote that he would be glad to read such a paper and, if it had not been previously published, he would try to have it appear in the *Annales* of the Institute. My *Utilitarian Economics* was written, but had not yet found a publisher, and on December 23, I commenced getting it ready to translate into French. I had many other "irons in the fire," and could only occasionally find a spare hour for this work, but I had it round all winter. On Christmas day I spent some time on it and decided to make a new English draft and call it *Pain and Pleasure Economy*, expanding the *Utilitarian Economics* and supplementing it with my eighth Hartford lecture on *The Subjective Trend of Modern Philosophy*. I put the two rough drafts together and interpolated the additional matter and commenced translating it on the same day. I then pegged away at it, and by January 26, 1897, the first part was translated and I attacked the lecture. It was all blocked out on February 10. My plan in writing French, as I have previously stated (see supra, Vol. V, No. 364), was to leave every other line blank and then submit it to our French professor for correction and idiomatic improvement. On February 11, I put it into the hands of Professor C. Fontaine. I wrote to Mr. McCormack on February 19, for the original of the quotation from Topinard on pp. 106–107, which was courteously sent me. I had several sessions over the manuscript with the French professor, who had no knowledge of science, but was a thorough grammarian. It was the middle of March before he was through, and then I proceeded, as time permitted, to rewrite the entire paper in final form. This was completed on April 15. After revising it I again submitted it on April 19, to Professor Fontaine, who returned it on April 22, with a few corrections which I made, and it was finished. I wrote a letter to Dr. Worms transmitting it, and mailed it to him on April 23. It was received and duly acknowledged and placed on the program of the Congress of the Institut that was to meet on July 21. I received this program on June 14, but I had stated that it would be impossible for me to attend.

On August 2, I received a postal card dated July 23, 1897, from M. Alfred Espinas in which he said: "Hier j' ai lu votre mémoire devant le congrès de Sociologie; il a été ecouté avec beaucoup d'attention et très applaudi." Dr. Worms also said in a note of August 7, "Votre travail a été lu intégralement à notre congrès, et y a produit fort bonne impression." The proof was received September 5, but the volume of the *Annales* (Tome IV) and my reprints did not appear till May, 1898. They were sent me May 2, and received May 9.

Among the public lectures that I gave at the University of

Chicago while conducting the summer courses in sociology in 1897, was one entitled *Pain and Pleasure Economy.* It was read on August 28, from the rough English draft of this paper.

Annales de l'Institut International de Sociologie, Paris, Tome IV, 1898, contenant les Travaux du Troisième Congrès tenu à Paris en Juillet 1897, pp. 89–134; Reprint [Extrait], covered brochure, Paris, 1898, pp. 1–44.

May 16, 1898—Ætat. 56.

455. A New Compass Plant

History.—Written December 3–4, 1897. The observations were made in September in the vicinity of Belvidere, Kansas. Good specimens of the plant were collected on September 19, and 26, which may be examined in the National herbarium. The last word of this article is misprinted "effected" for affected. In dictating, these words sound about alike and I find it so spelled in the duplicate (carbon copy) typewritten manuscript, which I have preserved. I dictated two other notes that day (see supra, Nos. 446, 448), done in haste in the midst of more important work, and overlooked this error.

The Plant World, Binghamton, N. Y., Vol. I, No. 3, May, 1898, p. 118.

September 9, 1898—Ætat. 57.

456. Review of Fossil Plants for Students of Botany and Geology. By A. C. Seward, M.A., F.G.S. With illustrations. Vol. I, Cambridge, 1898. Pp. xviii + 452. Cambridge Natural Science Manuals. Biological Series

History.—Written July 12, to August 24, 1898. I received a letter from Dr. Cattell dated April 21, 1898, asking me to review this volume, but I had met him at a reception in Washington the evening before and he had mentioned it and I had offered to undertake it. The book arrived April 26. I received a second copy from the author April 28, which I gave to Mr. David White. I read the book at home and finished it May 16, but I did not begin to prepare the review till July 12. I could not dictate this review, and I made a pencil draft of it on some soft blank letter-size paper that came in blocks or pads. I had written 17 pages of this when I had to go to Morgantown, W. Va., to give the summer courses in sociology, and from which absence I did not return till August 15. I had put this unfinished rough manuscript in a drawer in my desk at the Museum, to finish on my return. When I took it out August 20, I found that the mice had got into the drawer and gnawed the edges of all the sheets, so that I was obliged to have strips three inches wide pasted on and complete the missing parts from the context or by hunting up the data anew. Doubtless the gum that held the sheets together in blocks contained something that mice could eat, but they seemed to have eaten much of the paper itself. I finished the rough draft on August 23, and had it typewritten in duplicate. The first impression was mailed to Dr. Cattell August 24, and on August 26, I left for a field campaign in Kansas, from which I did not return until October 13. The proof came to the office September 5, and was read by Miss Schmidt and carefully compared and corrected.

I did not know then that Mr. Seward intended to write the second volume of this work, but I have recently (August 9, 1910) been asked to review it for *Science*, which I cannot do, having no facilities for this class of work here at Brown University.

Science, New Series, New York, Vol. VIII, No. 193, September 9, 1898, pp. 335–340.

October 29, 1898—Ætat. 57.

457. Descriptions of the Species of Cycadeoidea, or Fossil Cycadean Trunks, thus far determined from the Lower Cretaceous Rim of the Black Hills

History.—Written May 13, to July 2, 1898. This paper consisted of scarcely more than the part of my monograph on the Black Hills (see infra, No. 463) containing the descriptions of the species of Cycadeoidea. It is usually an advantage in systematic work to describe and publish new species in a preliminary paper, as species-mongers hang around ready to "crib" the descriptions from large monographs and claim the credit for them. Therefore when I had described these 21 species for that work and had the manuscript typewritten in duplicate I took the second impression (carbon copy) of those pages containing the cycad descriptions and submitted them for publication as a separate preliminary paper in the *Proceedings of the U. S. National Museum*.

In preparing this paper I made two mistakes in the names which I regret, but which cannot be changed according to the rules of nomenclature. I had the impression that Professor Macbride wrote his name McBride, and so wrote the specific name *McBridei*. I also gained the impression that Mr. Stilwell's name had *ll* in the first syllable, and so made the species *Stillwelli*.

The rule as to capitalization which I adopted in all my contributions to the U. S. Geological Survey publications was to give capital initials to specific names based on the names of persons, but not of places. The manuscript was so written, but the U. S. National Museum had adopted the rule of the zoologists to use no capitals in specific names, and mine were made lower case. My rule for the same names was followed in my final monograph (see infra, No. 463).

For all details of the work on the cycads of the Black Hills see the historical account of the monograph: *The Cretaceous Formation of the Black Hills as indicated by the Fossil Plants* (infra, No. 463). The present paper appeared on October 29, 1898.

Proceedings of the United States National Museum, Washington, D. C., Vol. XXI, published under the direction of the Smithsonian Institution, Washington, 1899, pp. 195–229; Reprint, brochure with cover and title-page, Washington, 1898, pp. 195–229.

January 27, 1899—Ætat. 57.

458. Review of Truth and Error, or the Science of Intellection. By J. W. Powell. Chicago, The Open Court Publishing Company

History.—Written December 26, 1898, to January 10, 1899. On November 17, 1898, Major Powell telephoned me and invited me to lunch with him in his office (Director's Office of the Bureau of American Ethnology). I went and he showed me ten copies of his new book, *Truth and Error*, dedicated to me. This was the first I knew of the dedication. I had dedicated *Dynamic Sociology* to him, and this was his opportunity to return the compliment. We had discussed much of its contents together, and the typewritten papers that he had been reading to the societies were all submitted to me to be read in advance. But I had never gotten his whole thought in one compact form. He inscribed a copy to me and I brought it home and immediately commenced reading it. I also wrote to Dr. Cattell that same day about reviewing it in *Science*. I said that in view of the fact that the book was dedicated to me I thought the review better not be signed. He replied November 22, saying that he did not think so, but I could do as I pleased in the matter, but he would be very glad to have the review. I took the book with me on a two weeks' trip to New England and finished reading it November 30. I then spent considerable time nearly every evening till Christmas compiling data from it for the tables that I prepared. It was an immense task, as Major Powell had neglected to bring his points together into such form that his system could be grasped by the reader. It was a great sacrifice for me, too, for I was earning two dollars an hour working on the *Supplement to Webster's Dictionary*, and the publishers were hurrying me up. I say this because, as will be seen, I have been criticised for writing this review. I worked for hours and hours correlating and tabulating his pentalogic categories. At last, on December 26, I was ready to write the review.

I made a rough draft, which I have preserved together with all my notes on slips of uniform size. This was completed January 4, 1899, and I then proceeded to rewrite it in final form. I mailed the manuscript to Dr. Cattell January 11, but before doing so I received a letter from him dated January 7, in which he said he had received a review of the book from Professor W. K. Brooks of Johns Hopkins University, and he was in somewhat of a quandary what to do. Of course I had nothing to say, and he finally decided to print them both, letting Brooks lead off and me follow. But Brooks's article does not go to the merits at all.

On Sunday, January 27, 1899, the next day after the article appeared, Major Powell called on me and we had a long talk about it. He said there were some things in his philosophy that I did not understand, which was undoubtedly true, but he was not at all offended by my criticisms, and never so much as intimated that he regarded them as unfriendly. He was the type of a true nobleman, and easily distinguished between a difference in our views and any personal difference. And this is perhaps the place to say that of all the men I have ever met he seemed to me the most truly great in his personal character. My relations with him down to the time when he voluntarily abdicated his position as Director of the U. S. Geological Survey, and contented himself with the directorship of the U. S. Bureau of American Ethnology in 1894, had been those of an official subordinate with an official superior, but he never made me feel that in the slightest. Still, I must confess that it made some difference in my general attitude towards him. He constantly sought to bring me closer to him, and I could have been very intimate with him if I had so chosen. But if there is anything in the world that I detest it is even the appearance of toadyism, fawning, or any form of chauvinism. It was this that kept me aloof a little, and all closer approaches emanated from him. As there were others who did not share my sentiments in this respect, and who were glad to be close to him, they were supposed to be the ones for whom he had the warmest attachment. Such persons naturally regarded my more distant manner as indicating a lack of esteem for the Major, but they were wholly wrong, for if there ever was a man whom I could truly say I loved, that man was Major Powell.

But we were always differing in opinion on certain minor points. I never allowed my personal attachment to influence my judgment on any of the subjects we discussed. This was so unlike the others by whom he was surrounded that he noticed it, and I am sure that he did not think any less of me for it. In fact, I know that he admired me for my independence and courage, and that he felt that I was useful to him in always giving him a wholly objective and unbiased opinion on any question. But when this review appeared the chauvinists were horrified. That I should dare to criticise Major Powell's views was an unpardonable offense. That I should do it in a book that was dedicated to me was considered as in bad taste. They were wholly incapable of distinguishing between philosophic disagreement and personal unfriendliness. He alone was utterly uninfluenced by any such considerations, and we continued to exchange visits and ideas as long as he lived.

But there was another reason for my giving the review the character that I did. When the book came out a number of my friends tried to read it for no other reason than that it was dedicated to me, and was hence supposed to reflect my views, or at least to receive my approbation and endorsement. But they found that they could not read it. It was so abstruse, abstract, and profound that it was wholly beyond them, and in such marked contrast, as they said, with the clear and simple way in which I led them to what seemed to be the same great truths, in so far as they could comprehend his conclusions. And they wanted to know of me if I really endorsed and defended the book as representing my own views and general philosophy. A purely analytical review would not have satisfied them, and would have left the wrong impression on their minds. This I really wished to prevent, and that was a strong motive both for my seeming criticisms and also for allowing the review to appear over my signature. But most of the letters that I received about this article laid stress on the uncommon fairness with which I treated Major Powell's ideas, which were not generally received with favor by scientific men.

Major Powell wrote a somewhat elaborate *Reply to Critics*, which he published in *Science* for February 17, 1899 (Vol. IX, pp. 259–263), aimed almost exclusively at the reviews of Professor Brooks and myself, in which, of course, I came in for the lion's share. It was friendly and free from everything personal, and it deals exclusively with the principles involved. I was quite willing to let him have the last word, and never returned to the subject in print. I will, however, draw attention here to one word, viz., "dynamism," which he applies to me, and which, as he intimated, did "offend" me, not because I disavow the theory or doctrine which he used the word to describe, but because the word had been used by others and also by myself for an entirely different theory or doctrine, which I reject. It is the "dynamism" of Leibnitz. the "centers of force" theory of Boscovitch, the "persistence of force" doctrine of Herbert Spencer, the "energetics" of Reinke and Ostwald. Major Powell might have avoided this error by simply glancing at my *Dynamic Sociology*, Vol. I, pp. 158, 221, where I characterize that school of scientific philosophers. But he seemed to think he was "coining" another new word to add to the many with which his book bristles.

Science, New Series, New York, Vol. IX, No. 213, January 27, 1899, pp. 126–137.

February 11, 1899—Ætat. 57.

459. Administrative Report for the year Ending June 30, 1897

History.—Written June 8–9, 1897. (See supra, Vol. V, No. 420.) This report appeared in February, 1899.

Eighteenth Annual Report of the U. S. Geological Survey, 1896–97, Part I, Washington, D. C., 1897 [1899], pp. 66–68.

March 15, 1899—Ætat. 57.

460. Sociology and Economics

History.—Written January 5–6, 1899. On March 26, 1897, I went to Philadelphia to attend the thirty-ninth Scientific Session of the American Academy of Political and Social Science, where Professor Sidney Sherwood read an elaborate paper. I had previously written out some remarks that I thought might be called for, as they were, and I supplemented them with others appropriate to the occasion. I preserved these notes, and nearly two years later, after Prof. Powers had published in the *Annals* of the Academy a remarkable series of papers dealing with the same general subject and quite a discussion had begun, I concluded to take part in it by working over my old notes and adding certain matter to them. This paper was the result. (For an earlier discussion of a similar question see supra, Vol. V, No. 379.)

Annals of the American Academy of Political and Social Science, Philadelphia, Vol. XIII, No. 2 [whole No. 51], March, 1899, pp. 230–234 [88–92]. Publications of the Academy No. 247 (covered brochure), Philadelphia, March 21, 1899.

July 13, 1899—Ætat. 58.

461. The Gospel of Action

History.—Written February 5–22, 1899. On January 7, 1899, I received a letter from Mr. Hamilton Holt, Office Editor of the New York *Independent*, requesting me to contribute an article on "some sociological subject" to be selected by myself. I had been reading Huxley's Romanes lecture on *Evolution and Ethics* and the *Prolegomena* in Vol. IX of his *Essays*, overlooked at the time they appeared, and had been struck with the similarity of his views, supposed to be original with him, to those I had been expressing long before the lecture was delivered, and I wrote to Holt proposing to write an article on *Evolution and Ethics, or the Law of Nature vs. the Law of Mind*, this secondary title being about that of Chapter XXXIII of my *Psychic Factors of Civilization*. He replied on January 11, that that title would be satisfactory, provided I could make it sufficiently popular.

It was three weeks before I could begin it on account of other work, and when at last I sat down on February 5, to block it out I decided to give it the title that it now bears. The rough draft was finished on February 12. There was another week's delay, and on February 19, I returned to it, revised it, and commenced making the final draft, which took only three evenings. In writing this article I felt that I was making it too personal and in my letter transmitting it I remember that I apologized for it and said that I hoped he (Holt) would like the article better than I did. On March 2, I received it back with a letter from William Hayes Ward, Editor-in-Chief, which, though couched in language calculated not to hurt my feelings, showed that the part that I disliked was objectionable, as he asked me to abridge the discussion of my personal relation to the doctrine. But he also wanted me to tack on a theological corollary, which he expressed in these words: "If man can so control and direct the laws and processes of evolution, may this not properly be done by a higher power, as Asa Gray used to say?" He begged me to make such modifications as I thought best and return the article.

I was a little discouraged, very busy, and somewhat indifferent to the whole matter, and I laid it aside indefinitely. But after more than a month had passed I received, on May 12, a letter from Holt saying that he had inquired of Dr. Ward about it and been informed that he had not heard from me. He wanted to know when he could have the article. I got it out and looked it over. I was not willing to append the theological codicil, but there was one paragraph, or part of one, that I was glad to expunge, and I proceeded to do so, and that was all the change I made. In order that the history of the article may be complete I will here introduce the words that I struck out as they appear in my rough draft. The next to the last paragraph on page 1868 had prefixed to it the following remark:

> "What struck me in these papers was of course their thorough agreement with my own published conclusions, sounding, as they do, almost like my own voice, using exactly the same arguments and to a large extent the same illustrations, so that many passages from both placed in parallel columns would present a most striking resemblance."*

In giving the history of my Rochester A. A. A. S. address on *The Psychologic Basis of Social Economics* (see supra, Vol. IV, No. 331), I have said something very similar to this.

Thus altered I sent it on again on May 14, 1899, and heard nothing more of it. I find no record of having seen a proof, and I think none was sent me. If I had corrected the proof I should probably have detected the error, where the reference is made to Chapter XXIII instead of XXXIII of the *Psychic Factors*, although I find that the error was my own and is so written in my rough draft of the article. Also I should not have allowed the editors to make the common mistake of saying "Smithsonian Institute" instead of "Smithsonian Institution," as they did

* A record that I keep shows that reprints of both my papers above mentioned were sent by me to Professor Huxley, the last one on January 16, 1893.

at the end of the article; but these words were not in the manuscript.

The Independent, New York, Vol. LI, No. 2641, July 13, 1899, pp. 1865–1868.

THERE is always danger that an important principle once established may be saddled with unwarrantable assumptions or supposed corollaries and pass into a sort of popular creed, unfavorably influencing men's activities and largely neutralizing the advantages of the general truth arrived at. It was so with the wholesome doctrine of non-interference with private enterprise—the *laissez faire, laissez passer* principle—put forth at a time when government was in the hands of a small ruling class bent on their own aggrandizement, and always true for many kinds of social activity, but which under democracy has become the bulwark of the so-called money power and the only hope of the classes in their effort to exploit the masses.

There are many analogous cases, but we are just now concerned with a great scientific principle, the one that has produced the most radical change that human thought has ever undergone—the principle of evolution—which the whole scientific world, with only minor individual qualifications, now accepts. This profound and far-reaching truth has given rise to an entirely new philosophy, much of it thoroughly sound, consisting of legitimate deductions from the facts of nature that have taught the world this principle, but other parts of it false, as, resulting from departures from the strictly scientific method, the introduction of illegitimate elements, and the elaboration of a mass of assumptions with which the facts are not at all in harmony. They are the products of the old propensity of the human mind to speculate far beyond the strict boundaries of concrete things and to construct systems largely independent of the real world.

I have in mind one of these deviations from the teachings of science manifested in the writings of certain truly great philosophers of our time who have taken the trouble to acquaint themselves with all the essential truths that modern science has unfolded. The particular fallacy embodied in this philosophy, which is fast becoming the basis of all the popular thinking of our day, is what, in analogy to one of the phases of theological thought, may be appropriately characterized as the modern *nature-worship*. The great lesson that science first taught to man was the lesson of nature. The truth embodied in it was stirringly shadowed forth even by Wordsworth:

"To the solid ground
Of Nature trusts the mind which builds for aye."

It became clear that nature was the chief object of study. From her bosom were being drawn all the rich blessings that science was showering upon the world. With this as a datum-point the speculative reason began to draw inferences. The first resulted in the injunction: Observe nature. Nothing could be more sound or wholesome. The second brought forth another injunction: Imitate nature. Nothing could be more false or pernicious.

While it cannot be complained that the first dictate of this proposed philosophy of science has not been obeyed, it is equally clear that the second has also been generally recognized and widely acted upon, and the result is that we are confronted with another huge error of the speculative faculty lying athwart the track of human progress, sullenly refusing to move, and powerfully obstructing the march of ideas and the advancement of the material interests of humanity.

The discoveries in astronomy, physics, and chemistry had made it clear that the physical universe was a domain of law, and not of the caprice of gods and demons. The vegetable and animal kingdoms had come to be studied also from this point of view, and the study of man had begun to take the form of a natural history science. Both the science of races, ethnology, and of industrial activities, political economy, acquired this character, and the idea of improving the condition of society was as little entertained as that of modifying the species of plants and animals that Linnæus and Cuvier had declared fixed and immutable. That natural history should pass through this statical phase is not to be wondered at, but even after Lamarck and Darwin had established the science of dynamic biology and shown the possibility of artificially improving the races of plants and animals, this idea still remained wholly foreign to the science of the human races and of society. Political economy still remained the same hopeless science that Malthus had made it; and down to the present closing years of the nineteenth century we find the leading philosopher of evolution, Mr. Herbert Spencer, treating sociology as a natural history science, and tho recognizing a secular genetic progress in human affairs, not only denying the power of man to accelerate that progress but deprecating as harmful all attempts in that direction on the part of society. His extreme views on this question have been aptly characterized by Prof. Ludwig Stein as the doctrine of a social nirvana.

The whole drift of modern philosophy relative to human affairs is to the effect that they are subject to natural laws that must not be interfered or "meddled" with. It is insisted that somehow this can do much harm, but that under no circumstances can it do any good; and the philosophers forget that the first of these declarations destroys their claim for the immutability of these laws and involves the admission of the possibility at least of affecting events favorably as well as unfavorably. I pointed out this flaw in the logic as long ago as 1883,* and undertook to show that the effort to control the course of natural phenomena in the interest of man and society embodied nothing inconsistent with the general teachings of science. I did not then go deeply into this question, but contented myself with demonstrating that in so far as science had really benefited man it had been through such modification and alteration of the course which natural phenomena would have otherwise taken, and that this is the principle underlying all useful art. It was clear to me even much earlier (and this it was which inspired my first work) that the current interpretation of the law of evolution amounted to a gospel of inaction and was as fatal to all initiative as had been either the optimism which asserts that there is nothing to do or the pessimism which declares that nothing can be done. It threatens to plunge the world into a lethargy as complete as that of the Middle Ages. The synthesis of the two half truths, optimism and pessimism, is meliorism. What there is true of both freedom and determinism is that every act is a cause which will produce its legitimate effect, and that this effect will not be produced unless the act is performed. Mr. John Morley has admirably expressed this idea in the following language:

> "It would be odd if the theory which makes progress depend on modification forbade us to attempt to modify. When it is said that the various successive changes in thought and institution present and consummate themselves spontaneously no one means by spontaneity that they come to pass independently of human effort and volition. On the contrary, this energy of the members of the society is one of the spontaneous elements. It is quite as indispensable as any other of them, if, indeed, it be not more so. Progress depends upon tendencies and forces in a community. But of these tendencies and forces, the organs and representatives must plainly be found among the men and women of the community, and cannot possibly be found anywhere else. Progress is not automatic, in the sense that if we were all cast into a deep slumber for the space of a generation, we should awake to find ourselves in a greatly improved social state. The world only grows better, even in the moderate degree in which it does grow better, because people wish that it should, and take the right steps to make it better. Evolution is not a force, but a process; not a cause, but a law. It explains the source and marks the immovable limitations of social energy. But social energy itself can never be superseded either by evolution or by anything else."†

Professor Huxley was almost the only prominent man of science who saw the evil to which the kind of philosophy above described was leading, and in his lecture on "Administrative Nihilism," 1871, he dealt it some very heavy blows, but he did not there enter deeply into the argument, and contented himself with pointing out some of its logical absurdities. This had little effect in stemming the tide of thought in the direction of a thoroughly sterile philosophy, and no one seemed to wake up to a realizing sense of the true situation.

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* "Dynamic Sociology," vol. I., p. 36.

† John Morley "On Compromise." I find this extract in that excellent little collection entitled "Voices of Doubt and Trust," selected by Volney Streamer [George Iles]. New York, 1897, pp. 124, 125.

So deeply was I impressed with this state of things that in 1884 I formulated a protest against it as strong as I was then able to make it, and with a view to landing it as nearly as possible into the heart of the philosophical camp I published it in the English psychological journal, *Mind.** In this paper I showed that the gradual growth of the rational faculty had introduced an entirely new element into all economic operations, to a great extent reversing the conditions that obtain in the animal world and in the unthinking masses of mankind, and rendering most of the laws of political economy that had been laid down by the earlier economists wholly inapplicable to the modern social state.

During the eight years that followed this scarcely a voice was raised against the current tendencies. Mr. Spencer's philosophy was gaining ground as volume after volume emerged from the press and was devoured by the reading public. Its inadequacy to meet the changing conditions of social life was more and more apparent to my mind, and I at last made a renewed effort, not so much to refute it as to probe the question to the bottom, and to arrive if possible at a theoretical basis for the opposite class of views which were being freely expressed by a large class of social reformers who possessed few of the scientific qualifications for answering the argument, and from whom there was almost as much to be feared as from the philosophers themselves. It was clear that there were two principles at work, both psychic in a broad sense, but the one which had been alone previously attended to, based on the ever-recurring wants of man, prompting him to seek satisfaction through direct efforts to attain his ends, and therefore relatively simple and easily reducible to economic laws, and the other based on his intelligence, calculation, foresight, and cunning, which are subtle elements and difficult to deal with scientifically, and the increasing prominence of which was constantly baffling the economists and throwing their schemes into confusion.

All this I elaborated into what seemed to be a somewhat complete philosophy of mind, individual and collective. It is embodied in my "Psychic Factors of Civilization," which appeared in the autumn of 1893, but more than a year earlier I presented to the public the general conclusions arrived at in the form of an address as vice-president of the Section of Economic Science and Statistics of the American Association for the Advancement of Science at its meeting in Rochester in August, 1892, and it appeared in the "Proceedings" of the Association for that year* under the title: "The Psychologic Basis of Social Economics." To give these conclusions as wide a vogue as possible I also published this address, slightly condensed but under the same title, in the "Annals of the Academy of Political and Social Science" for January, 1893.† In a much expanded form it constitutes Chapter XXXIII of the "Psychic Factors," which is entitled "The Economy of Nature and the Economy of Mind."

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* "Mind as a Social Factor." *Mind*, vol. IX., October, 1884, pp. 563–573.

These papers contain, so far as I am aware, the only attempt that has been made to formulate what I call "the law of biologic economics," and to contrast this with "the law of mind," or the method by which intelligence works in human affairs. The total failure of all economists to note this fundamental distinction is the sufficient cause of the glaring discrepancies between economic theory and historical fact, and it is this which has not only brought political economy into disrepute, requiring its very name to be changed to *economics*, but has caused the rise of an entirely new school of economists, whose teachings are to a large extent diametrically opposed to those of half a century ago.

It was in 1893 that Professor Huxley delivered his famous Romanes Lecture on "Evolution and Ethics," in which he struck the same key-note, and it was in 1894 that he wrote the still more remarkable "Prolegomena" to that lecture, which precedes it in the ninth volume of his "Collected Essays." These masterly documents have been the subject of almost unnumbered criticisms in the public press, often from the pens of shallow and flippant reviewers, but sometimes of able and responsible thinkers; and altho there has usually been an attempt to qualify or even overthrow Huxley's position, this always seems faint and feeble when placed by the side of the terse and vigorous utterances of that "prince of debaters," as Matthew Arnold called him.

But Professor Huxley, besides the weight of his name in giving currency to his views, had the tact to connect the subject with that of ethics, with which it really has nothing to do, doubtless perceiving that this catchword in the title would commend it to many who would never read a purely scientific article.

These papers by Professor Huxley have undoubtedly done more than anything else that has been written to set the world thinking in the right direction upon the true teachings of science relative to human and social affairs. They show that to the influence of intelligence in artificially modifying the environment of man in his own interest there is scarcely any limit, and that all that the world stands in need of is vigorous action in this direction. Nature is indeed first to be observed, but, having been learned, the laws of nature become the property and servants of man, and he is not to imitate the methods by which nature accomplishes results, but must direct the forces of nature into channels of his own advantage, and utilize for his own good all the powers of the universe. It is not the doctrine of inactivity, of the folding of the arms, of *noli me tangere*, of *laissez faire*, that naturally and legitimately flows from a full comprehension of the law of evolution, but a gospel of action, a recognition of the law of causation and of man as a great and potent cause in the world. The true crown of a system of scientific philosophy is not an Ethics which seeks to restrain and circumscribe activity, but a Sociology which aims at the liberation of action through the directive agency of intelligence.

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* Vol. XLI., 1892, pp. 301–321.

† Vol. III., No. 4, pp. 464–482.

Smithsonian Institution, Washington, D. C.

November, 1899—Ætat. 58.

462. Administrative Report for the year Ending June 30, 1898

History.—Made out at the end of the fiscal year in the usual way. (See supra, Vol. V, No. 420.) This report appeared in November, 1899.

Nineteenth Annual Report of the U. S. Geological Survey, 1897–98, Part I, Washington, D. C., 1898 [1899], pp. 63–65.

November 22, 1899—Ætat. 58.

463. The Cretaceous Formation of the Black Hills as indicated by the Fossil Plants

History.—Written March 7, to July 8, 1898. Revised and additional matter inserted October 17, to November 2, 1898. Further additional matter, chiefly sections furnished by Professor Jenney, introduced December 1–5, 1898.

My interest in the geology and fossil flora of the Black Hills Cretaceous dates back to March, 1893, when Mr. F. H. Cole sent the beautiful specimen of *Cycadeoidea dacotensis* to the U. S. National Museum (see supra, Vol. IV, No. 344). My first visit to the region was made in September, 1893, and the results of my work there were published in the spring of 1894 (see supra, Vol. V, No. 365). My second expedition to the Black Hills was made August 19–21, 1895, at which time, in company with Professor Jenney, further studies were made especially of the stratigraphy, including that of the overlying Dakota Group, and collections of fossil plants were made at many localities. My third visit to these parts was made in October, 1898, when, in company with Mr. H. F. Wells, a great number of cycad localities were seen with the trunks lying on the ground. The history of all these expeditions and that of our knowledge of the geology and paleontology of the Black Hills Cretaceous are given in full detail in this monograph. I shall therefore confine myself here to giving an account of its preparation, including the description of the material.

This may be said to have been begun on February 21, 1898, when I commenced working up the bibliography of the subject, and decided upon a proper title, which was never changed after that date. My plan in this, as in all previous and subsequent papers, was first to acquaint myself thoroughly with all that had been already done, so as to start from where others had left off, and thus make my work a fresh contribution to the subject. But to make this clear it was necessary to write a historical introduction chronologically arranged, showing exactly what had been done, when and by whom. This involved a great amount of literary research; every book referring to others must be hunted up and read, its literature sought out in turn, until the entire field was exhausted, and I was sure nothing had been overlooked. This method was in such marked contrast with that of other members of the staff, and called for so many books from the libraries that the librarians told people that I got all my information from books, whereas the whole object of it was to avoid taking anything from books; a good illustration of how reputations are made. How thorough I was in this method is indicated by the citation from Poe's *Thousand-and-Second Tale of Scheherezade* (see title page to this volume). Such literary embellishments of scientific memoirs are usually spurned by specialists, who glory in the bare unreadable statement of facts.

This bibliographical preparation was completed on March 3. The collection of cycads that had been made from the Black Hills and was in the National Museum was photographed by Mr. Smillie, the Museum photographer, under my careful supervision on March 5, and these handsome photographs were used as illustrations. On March 7, I commenced the dictation of the preliminary and historical part, which was finished on March 16. On the 18th the chapter on the Minnekahta region was begun, and this was completed on the 21st. On March 18, I received a letter from Professor Marsh stating that he had a large collection of cycads from the Black Hills which he would like to have me come to New Haven and describe. There was considerable mystery connected with this. Some time previously, when he was in Washington he had manifested great interest in the beautiful specimens of cycadean trunks that the Museum had acquired, all of which I showed him with considerable pride on account of my part in securing them. He questioned me quite closely about where they were found and took notes on a pad as I explained. I fancied he would try to get specimens for Yale to rival the Museum, but I felt that this would be a good thing, as the

Museum had no funds to devote to that purpose, and I had gone as far in my Survey work in that line as I thought consistent with the purposes of a Geological Survey. But I did not know any other localities in the Black Hills than those I had visited, nor any one living there who did know such.

As I was an officer of the Geological Survey I could not go and describe cycads at Yale without authority from the Director, and if the Survey had this work done it could claim the right at least to publish the results, if not to a share of the material. I showed the Director Professor Marsh's letter, and he told me I had better go at once, and offered to allot me the amount of my expenses. I received orders on March 21 and went the next day. I found he had 86 specimens, good, bad, and indifferent, but many of them very fine. Though very communicative about trifles, Professor Marsh was reticent as to the source of the material, and I could learn nothing. But I knew they were from the Black Hills, and some were of the same species as those of the Museum. I proceeded at once to describe them. I was then skilled in cycad descriptions having been making them for over three years, first for those from Maryland (see supra, Vol. V, Nos. 362 and 433), and I had also described the Museum specimens, but had not yet published the species which I had named and was keeping secret. By March 29, all the Yale specimens were described, and a photographer was secured, who, under my direction, photographed them all on that and the two following days. On March 30, practically at my demand, Professor Marsh at last condescended to give me roughly the localities where the trunks were found, but he would tell me nothing else. I returned to Washington on March 31. I had made descriptions of specimens only, and it remained to correlate the characters and see how many distinct species were represented. Professor Marsh was very anxious to know this, and also how many were distinct from those at the Museum.

I then divided my time between dictating my paper and working out the cycad descriptions. The photographs were sent me on April 15. By April 16, I had described all the species and assigned the specimens to each, and had named all but one. As soon as I returned from New Haven I wrote to Professor Jenney and told him all the circumstances, asking him if he could throw any light on the mystery. On April 18, I received a letter from him telling me who sold Professor Marsh the specimens. It was Mr. H. F. Wells of Sturgis, S. D., and I immediately wrote to him for information relative to localities, etc. On May 2, I received a letter from him saying that he was not at liberty to tell me anything about the cycads, and referring me to Professor Marsh. On May 4, I saw the Director and told him all about the trouble. I urged that the Survey should refuse to go on with the work of publication till full information should be furnished on all points. He authorized me to write him to that effect, and I did so. On May 6, I received his reply. He was most gracious and offered me every facility for completing the work. In the same letter he stated that another invoice of cycads was on its way and he wanted me to come again and describe them also.

Meantime Professor Jenney had sent on the data for writing up the Hay Creek region, and I proceeded with that chapter. The geographical and geological part was completed in May, including maps and sections, and I was ready to treat the fossil floras. I decided to begin with the fossil cycadean trunks, as the part which was most peculiarly my own. But as it was evident that a year or more must elapse before these descriptions and illustrations could see the light in this form I decided to publish a preliminary paper giving the descriptions of all the species then known, and thus securing their permanence to science. This was accordingly done and this paper is treated under another head (see supra, No. 457). I named the one unnamed species for Mr. Wells, with whom I was now in full communication, Professor Marsh having instructed him to give me all the information I asked for.

Professor Jenney's collection from the Hay Creek region was sent to Professor Fontaine as soon as it was received and described by him, but I worked out the classification and synonymy and did much other work on it. Dr. Knowlton worked up the few specimens of silicified wood and his report appears as his own. I described the species from the Dakota Group, with which the memoir closes. The great table of distribution at the end was prepared by me at immense cost of time and labor.

Professor Marsh's second invoice of cycads arrived in due time and I went again to New Haven on June 6, and described them. They were an inferior lot, mostly fragments, but there were a few very perfect ones, and one immense branching trunk, which proved to belong to the species that I had named for Professor Marsh. I completed the descriptions on June 11, and superintended photographing all the important specimens. I returned to Washington on June 14. I embodied all the new matter in my preliminary paper. The last lot yielded four new species.

The remainder of June was devoted to engrossing the rough, dictated manuscript in a perfect duplicate typewritten copy and in mounting and describing the plates. It was July 8 before all was done, and on that day the first impression, the roll of maps, and the package of mounted plates each accompanied by its description on a special sheet, were sent to the Editorial Division of the Survey.

I was absent from Washington most of the rest of the summer, and did not return till October 13, but the last part of this time was spent in a side trip from Kansas, where I had been since September 1, to the Black Hills, where I had arranged with Mr. Wells to meet him and accompany him to the cycad localities. This was from October 3 to 9, and we made an extensive tour of the Hills. He had found many trunks that he had not sold to Professor Marsh, and the latter had declined to buy any more. He wished me to help him dispose of them. I saw them all, and many were superb. I promised to try to induce the National Museum to acquire them, and he offered to collect and ship them all for $100. This, as I could see, was very reasonable, and it would have given the Museum a collection of cycads that would compare well with that of Yale. When I got back I urged these considerations upon the Museum authorities, but without success. Funds were low and cycads were not appreciated. The result was that Professor Marsh ultimately got the collection, which makes the Yale collection of cycads a truly wonderful one (see infra, No. 472).

A number of things had occurred since I sent in the manuscript to make me want to see it again before it went to press, and as soon as I got back I called at the Survey to see if it was still there. I found it in the Editorial Division and had it sent over to the Museum. On October 17, I commenced work on it and put in a good deal of additional matter at various points, including a number of new geological sections. The maps, too, were imperfect, and I had much new matter to put on them. I indicated on the general map the towns and railroads, and marked off the boundaries of the Minnekahta, Blackhawk, and Hay Creek regions. This work was completed on November 1, and the manuscript returned to the Survey on November 2.

On December 1, I received from Professor Jenney his sections of the Hay Creek region which did not accompany his original report. I had begged him for them and was glad to get them before it was too late. They were imperfect and had to be copied, which took several days, but I looked after the matter at once and found that the manuscript had gone to the printer, and the large chart would have to be inserted in the galley proof. The columnar section, however, must pass through the Division of Illustrations, and the rest of the illustrations were still there, so when completed I sent it to that Division.

Though the last to go in, the illustrations were the first to reach me in the proof, which was on January 21, 1899. They were carefully corrected and returned on January 26. The galley proof of the letterpress began to come on February 6. The interested reader may have noticed that the monograph describes one more species of Cycadeoidea than does the preliminary descriptive paper, and if he looks further he will find that the additional species is *C. Wielandi*. Under that species the history of its discovery is given, but it is not there stated that the discovery was made too late for insertion in the manuscript. This was done in the galley proof on February 10, 1899. The galley proofs were not all read till March 8, and the page proof arrived on March 25, and this hung on till April 4. Then a second revise on April 13, making four more days' work. Of this I retained a complete set, and on April 17, I commenced making the index from it. This took till May 1. Proof had to be read, May 27, of the lettering and legends of the plates, then that of the index had to be read on June 13, and page proof of the same a little later. I mention all these details, not because they have any special interest, but to show what an elaborate undertaking it is to publish a memoir of this character.

In this case there was less than the usual delay in bringing out the volume, which appeared in December, 1899. My reprints, however, reached me on November 22. They were superb, and I distributed them far and wide, sending many abroad through the International Exchange Division of the Smithsonian Institution. In due time I began to receive acknowledgments and a great number ultimately came. I have just been looking them over, and I find that they nearly all consist simply of empty praise, scarcely any of the writers having more than glanced at the work. How could it be otherwise? How many even of the leading geologists and paleontologists of the world had any special knowledge of such a subject? In a few cases I inadvertently sent copies to sociologists. Their letters were amusing. This memoir simply "made their eyes stick out." Supposing that I was devoting my whole life to sociology (and many had expressed their surprise that I could do so much work!) they were simply amazed to see that my real vocation was geology and that sociology was only my avocation.

Ibid., Part II, Washington, D. C., 1899, pp. 521–958, pl. liii–clxxii.

January 5, 1900—Ætat. 58.

464. The Fossil Forests of Arizona

History.—This is simply Mr. F. A. Lucas's report as Secretary of the Society of my oral remarks made December 16, at the Biological Society of Washington, but on December 19, he brought them to me and read them and I suggested a few changes. The whole subject will be fully treated in the history of my Report on the Petrified Forests of Arizona (infra, No. 465). The chief interest attaching to this paper grows out of the presence of Ex-Governor W. A. Richards (of Wyoming), then Assistant Commissioner of the Land Office, who was interested in the subject, and who at my suggestion was invited to speak. The following is the Secretary's report of his remarks:

Ex-Governor W. A. Richards, Assistant Commissioner of the General Land Office, who was present and was invited to speak, said that the Land Office was in earnest in this matter and was glad to have a truly scientific report on the subject. He stated that a very large amount of the material was now being worked up in this country into articles to be sold at the coming Paris Exposition.

Science, New Series, New York, Vol. XI, No. 262, Jan. 5, 1900, pp. 30–31.

AT the 314th meeting on December 16th, Lester F. Ward spoke of the *Fossil Forests of Arizona*, saying that they were of Mesozoic age, the strata containing the petrified trunks ranging from just above the Permian up to and including the Upper Trias; the formation extended northwards into Utah. The territory in which the petrified forest lay was in Apache Co., east of Holbrook, but while the trunks were found over a very considerable tract the best portion of the "Forest" was embraced in an area about eight miles square. In some portions of this the petrified logs lay much more thickly than they could have stood when living. In fact these trees did not lie where they had grown, but had been transported thither in Mesozoic time by strong and swift currents and had then been rapidly buried in sand. The trees were completely silicified and so well preserved that the microscopic structure could be clearly made out, showing that they were related to the Araucarian Pine of the Southern Hemisphere; hence the genus had been named *Araucarioxylon*. The speaker stated that his recent visit to the petrified forest was the result of a request from the General Land Office for a report as to the desirability of reserving the most interesting portion as a national park, a memorial to Congress to that effect having passed the Legislature in 1895. Owing to the visits of tourists, the more beautiful specimens were being steadily carried away and destroyed, while many carloads had been removed to be cut, polished, and made into ornaments. Owing to the extreme hardness of the silicified trunks, it had been proposed to utilize them in the manufacture of a substitute for emery, and a crushing plant had actually been erected, although never operated, owing to the development of the corundum industry in Canada.

February 14, 1900—Ætat. 58.

465. Description of a New Genus and Twenty New Species of Fossil Cycadean Trunks from the Jurassic of Wyoming

History.—Manuscript and plates prepared December 1–2, 1899. As this is only a preliminary descriptive paper taken from my large monograph on "The Status of the Mesozoic Floras of the United States, First Paper" (infra, No. 471), all details will be given in connection with that paper, and I will confine myself here to a brief account of its separate publication. The same reason existed for doing so as in the case of the Black Hills cycads (see supra, No. 457). The Washington Academy of Sciences had just been founded, and it proposed to publish extensively. I thought I would publish the paper there instead of in the Proceedings of the U. S. National Museum, as in the previous case. The manuscript was nearly all written and the plates mounted for the monograph, so that it was merely a matter of selecting and arranging them. The results of my field work in Wyoming in September had not been written up, and these I dictated on December 2, and sent to the proper officer of the Academy on that day. Proof of the plates was received on December 27, that of the text on January 3, 1900, and page proof on the 8th. The Academy adopted the brochure plan, and my paper is dated February 14, 1900. In fact, I received the number ordered on that day. I distributed about 100 at once. The rule of the Academy, as of the Museum, was to use no capital initials for specific names.

Proceedings of the Washington Academy of Sciences, Washington, D. C., Vol. I, February 14, 1900, pp. 253–284, pl. xiv–xxi. Brochure with cover lettered as title page.

February 16, 1900—Ætat. 58.

466. Review of Social Laws. An Outline of Sociology. By G. Tarde. Translated from the French by Howard C. Warren, Assistant Professor of Experimental Psychology in Princeton University, with a preface by James Mark Baldwin. New York, The Macmillan Company. 1899. Small 8°. Pp. xii + 213

History.—Written January 21–24, 1900. I received the book from Dr. Cattell on December 16, 1899, with request to review it for *Science*. I was familiar with the work, having read the original French in 1898, soon after it appeared. But I also knew all of Tarde's works. So I needed only to glance through the translation to see whether it was well done. My eye caught several defects, but I pointed out only one. As there was little to say, and as this little book is a sort of summary of Tarde's whole social philosophy, I thought it a good occasion for setting that forth, and especially for pointing out the biological homologies of Tarde's principal terms. This had never been done, and needed to be done. I even pointed out the physical or cosmical homologues. The only improvement that I would now suggest would be to say that Tarde's "adaptation" comes very close to my "synergy."

I received a note from Tarde, to whom of course I sent the review, which I think might well be introduced here to show his attitude on the whole subject:

"Monsieur:—Je vous suis très obligé de l' article si bienveillant et en même temps si exact dans l'exposé de mes idées, que vous avez bien voulu considérer dans *Science*, à mon livre sur les *Lois Sociales*, et à la traduction qui m' a été faite par M. Baldwin. Je suis heureux de voir mes théories discutés dans votre pays où la sociologie trouve à la fois un champ d'application et d'observation si merveilleux et une curiosité singulièrement éveillée à l' égard des idées sociales."

From the translator I received a very humble letter thanking me for pointing out his "blunder" relative to the word "machinofacture."

Science, New Series, New York, Vol. XI, No. 268, February 16, 1900, pp. 260–263.

April 13, 1900—Ætat. 58.

467. Report on the Petrified Forests of Arizona

History.—Written December 4–12, 1899. On June 28, 1899, I was requested to assist in drafting a reply to the letter of the Commissioner of the Land Office, Mr. Binger Hermann, printed on page 6 of this report. Dr. F. H. Knowlton and I are responsible for most of the letter that was prepared, and which is printed on pages 7 and 8 of my report. The paragraph on page 8 referring to me was added by Mr. Rathbun, I having offered, if requested, to undertake the investigation on my return from my prospective campaign on the Pacific Coast. Before leaving Washington in August I went to the Land Office, and in the absence of Commissioner Hermann, I saw Gov. Richards who expressed great interest in the matter, and gave me a copy of the Memorial No. 4, printed on pages 5 and 6 of my report. He also wrote to the Director of the U. S. Geological Survey, asking him to give me instructions to make the investigation.

While I was in the field in Oregon I received from President Jordan of Stanford University an invitation to give a course of lectures on Sociology in October, which I accepted, and after my field work was finished I went there and gave the lectures. As soon as I reached Stanford University I wrote several letters to persons in Arizona whose names appear on the above-mentioned Memorial, for I had no other names, and although the document was four years old, I hoped some of the persons might still be there and give me the desired information. My letter to Secretary Bruce, of Arizona, in 1895, brought me an answer from his successor, Mr. C. H. Akers, but it contained none of the desired information. From the others there was no reply. When, therefore, I was ready to visit Arizona on the first of November I had nothing but my instructions from Washington to go upon.

I did not go to Phœnix at all, but remained in the region of the petrified forest and in the valley of the Little Colorado till the middle of November, making a pretty careful study of the geology and of the nature of the silicified and agatized trunks. This report is based upon that investigation, and although a much more thorough one was made in 1901, and some errors corrected, the description here given will stand as in the main correct.

I left Arizona on November 16, and returned to Washington. It happened that in the same car with me were Messrs. George W. Sturtevant and Frank M. Murphy, whose acquaintance I made, from which resulted the facts recorded on pages 19–22 of my report. I wrote the report as stated, and sent it to the Director of the Geological Survey, not knowing what use he would make of it. He referred it to the Secretary of the Interior, with the following letter of transmittal.

Department of the Interior,
United States Geological Survey,
Washington, D. C., December 14, 1899.

Sir: I have the honor to transmit herewith a report by Prof. Lester F. Ward on the Petrified Forests of Arizona.

The request for the investigation and report came, at the instance of the Secretary of the Smithsonian Institution, from the Hon. Binger Hermann, Commissioner of the General Land Office. In order that the origin of the report may be fully understood by the Department, Professor Ward has included a copy of the correspondence leading to it.

Yours respectfully,
Chas. D. Walcott, *Director.*

The Secretary of the Interior.

Mr. Edward M. Dawson, Chief Clerk, was specially interested in it, and I surmise that he had much to do with its publication. Proof reached me on March 23, 1900, and it came out on April 13. I received 200 copies for my personal use.

On March 19, the Director telephoned me that the Committee on Public Lands would probably soon send for me to come before it and make a statement relative to the Petrified Forests of Arizona. On the 27th Mr. John F. Lacey, Chairman of the Committee, called on me and we discussed the subject. He asked me to come before his Committee the next day. Unfortunately I had made an engagement to deliver a lecture in Indianapolis that evening and could not have got there in time if I met the Committee. I offered to cancel the engagement by telegraph, but he said I need not do so. My report had not yet appeared, but I had a duplicate typewritten manuscript, and it was arranged that Dr. Knowlton go in my place and take the manuscript. This answered every purpose, and on April 3, I received from Mr. Lacey a copy of "A Bill (H. R. 9634) To set apart certain lands in the Territory of Arizona as a public park, to be known as The Petrified Forest National Park," together with his report, in which he had quoted extensively from my paper. Considerably more than half of the original matter is embodied in his report, and this appeared about ten days in advance of the report itself.

On April 8, a reporter for the New York *Tribune* interviewed me on the subject and the *Tribune* for April 15, contains a somewhat extended account; practically what I dictated. On April 9, I received the following:

Committee on Public Lands.
House of Representatives, U. S.
WASHINGTON, D. C. April 8, 1900.

MY DEAR MR. WARD:

The Petrified Forest Bill passed the House to-day.
Be sure and see the Senate Committee.

Yours truly,
JOHN F. LACEY.

I took this letter the next day to Gov. Richards, who said he was going to the Capitol in the afternoon and would speak to members of the Senate Committee about it. On April 20, I went to see Senator Mason myself, and he said he would urge the passage of the bill, but I did not know many Senators and could do little in the matter. I believe it failed to come up in the Senate at all, but I never heard any details. It certainly never became a law. It was, however, revived the next session, and a letter from the Secretary of the Interior to the Director of the Geological Survey relative to the House Bill, which it said was then pending in the Senate, was referred to me on January 9, 1901. I never heard what came of it. Mr. Geo. F. Kunz, the well-known gem expert, was deeply interested and wrote me several letters offering to do anything he could to further the matter. He brought it to the attention of the American Scenic and Historic Preservation Society, and its Eighth Annual Report to the Legislature of the State of New York, transmitted on

April 22, 1903, pp. 74–79, contains a powerful appeal for the passage of Mr. Lacey's bill, made by a special committee on Petrified Forest National Park, consisting of Messrs. Andrew H. Green and George F. Kunz. Senator Depew is quoted as favoring this action. My report is quoted, some additional ground gone over, and two views of the petrified forest introduced, one of which is the Natural Bridge.

On November 2, 1900, I learned that Mr. Richard Rathbun, Assistant Secretary of the Smithsonian Institution, had asked permission to republish my report in the Appendix to the Smithsonian Report. I was consulted in the matter, and it was proposed to introduce a few illustrations. I had obtained a number when in Arizona, but had seen better ones, and I advised applying to Mr. Adam Hanna of Adamana for some. This was done and three views were secured, in one of which the Natural Bridge is well shown with Mr. Adam Hanna standing on it. It was printed in the Smithsonian Report for 1899 with these three illustrations, and on my return on June 28, 1901, from my second and more extended visit to Arizona I found copies of the reprints on my desk. It was printed from the same plates as the original report, which had been preserved at the Government Printing Office, the only change, besides the insertion of the three plates, being that of the page numbers. The following footnote from the title stands at the bottom of the first page:

> Reprint of Report on the Petrified Forests of Arizona, by Prof. Lester F. Ward, to the Director of the U. S. Geological Survey, published by the Department of the Interior. Further removal of the fossil trees has been restricted under regulations of the Interior Department.

I do not know who was the author of this note nor what authority he had for the statement contained in the last sentence.

The reprint is also a covered brochure.

Report on the Petrified Forests of Arizona, by Lester F. Ward, Paleontologist, U. S. Geological Survey. Special publication of the Department of the Interior, brochure with cover and title page, pp. 1–23. Washington Government Printing Office, 1900; Annual Report of the Board of Regents of the Smithsonian Institution for the year ending June 30, 1899, Washington, 1901, pp. 289–307, pl. i–iii; Publications of the Smithsonian Institution, No. 1233, reprint with cover and title page, Washington, 1901, pp. 289–307, pl. i–iii, 56th Congress, 1st Session, House of Representatives. Report No. 879. Petrified Forest National Park, of Arizona. Report to accompany H. R. 9634, pp. 2–5.

December 12, 1899.

SIR: In compliance with your instructions dated October 9, 1899, directing me "to visit what is known as the 'Petrified Forests of Arizona,' and, upon your arrival in Washington, render a detailed report of your investigations and observations concerning the same, including such information as may be of value touching the proposition to set aside the region embracing the Petrified Forests as a national park," I have the honor to make the following report:

In order to place the subject in as clear a light as possible, I will first give a brief historical account of the recent movement in favor of making a public reservation of the region embracing the Petrified Forests of Arizona.

In 1895 the legislative assembly of the Territory of Arizona adopted the following memorial to Congress:

HOUSE MEMORIAL No. 4

To the Senate and House of Representatives of the United States of America in Congress assembled:

We, your memorialists, the eighteenth legislative assembly of Arizona, beg leave to represent to your honorable bodies:

First. That there is in the northern part of this Territory, lying within the borders of Apache County, near the town of Holbrook, a wonderful deposit of petrified wood commonly called the "Petrified Forests" or "Chalcedony Park."

This deposit or forest is unequaled for its extent, the size of the trees, and the beauty and great variety of coloring found in the logs.

The country 10 miles square is covered by the trunks of trees, some of which measure over 200 feet in length and from 7 to 10 feet in diameter.

Ruthless curiosity seekers are destroying these huge trees and logs by blasting them in pieces in search of crystals, which are found in the center of many of them, while carloads of the limbs and smaller pieces are being shipped away to be ground up for various purposes.

Second. Believing that this wonderful deposit should be kept inviolate, that future generations may enjoy its beauties and study one of the most curious and interesting effects of nature's forces,

We, your memorialists, most respectfully request that the Commissioner of the General Land Office be directed to withdraw from entry all public lands covered by this forest until a commission or officer appointed by your honorable bodies may investigate and report to you upon the advisability of taking this forest under the charge of the General Government and making a national park or reservation of it.

It is annually visited now by hundreds of scientific men and travelers from every State and country, and some such action by your bodies would preserve it from the vandalism it is now subjected to.

We would further state that at present there is no person living within the limits of the proposed park, so that no settlers will be disturbed by any such action on your part.

And be it resolved by the legislative assembly of the Territory of Arizona, That our Delegate in Congress be, and is hereby, instructed to use all honorable means to have some action taken by Congress to have this Chalcedony Park set aside and formed into a national park under the care and charge of the General Government.

Also that the secretary of the Territory be, and is hereby, requested to transmit a copy of this memorial to each House of Congress, our Delegate to Congress, and the United States Land Commissioner.

J. H. CARPENTER, *Speaker.*
A. J. DORAN, *President.*

[Indorsed]

I hereby certify that the within memorial originated in the House and is known as House Memorial No. 4.

CHAS. D. REPPY, *Chief Clerk.*

Filed in the office of the secretary of the Territory of Arizona this 11th day of February, A.D. 1895, at 11 A.M.

CHAS. M. BRUCE, *Secretary of Arizona,*
By F. B. DEVEREUX, *Assistant.*

In June last the secretary of the Smithsonian Institution received the following letter from the honorable Commissioner of the General Land Office:

PETRIFIED FOREST, ARIZONA.

DEPARTMENT OF THE INTERIOR, GENERAL LAND OFFICE,
Washington, D. C., June 17, 1899.

SIR: I am in receipt of a certified copy of a memorial by the legislature of Arizona praying that certain lands in Apache County, Arizona, in the vicinity of the town of Holbrook, known as the "Petrified Forest," be withdrawn from entry with a view to creating a reservation or national park for the purpose of preserving the natural wonders and curiosities of the same.

I have the honor to request that you will kindly inform me whether the records of the Smithsonian Institution furnish any information respecting this locality indicating that the scenic features of the same are of such a nature as to render it desirable, in the interest of the public, to set these lands apart as a national park. I will be pleased to receive a full expression of your views on this subject, and also as to the importance of preserving the mineralized formations in that region.

Very respectfully,
BINGER HERMANN,
Commissioner.

The SECRETARY OF THE SMITHSONIAN INSTITUTION.

To this letter the following reply was made:

SMITHSONIAN INSTITUTION,
Washington, D. C., July 7, 1899.

SIR: I have the honor to acknowledge the receipt of your communication of the 17th ultimo requesting information concerning the Petrified Forest near Holbrook, in Arizona, as well as an expression of opinion concerning the desirability of setting aside these lands as a national park, and beg to furnish the following statement:

The region near Holbrook, Apache County, Arizona, known as the "Petrified Forest," "Chalcedony Park," and "Lithodendron (stone trees) Valley," is of great interest because of the abundance of its beautiful petrified coniferous trees, as well as of its scenic features. The trees lie scattered about in great profusion, but none stand erect in their original place of growth as do many in the Yellowstone National Park. The National Museum possesses three splendid trunks collected there by Lieutenant Hegewald at the request of General Sherman.

The best popular account of this region is given by Mr. George F. Kunz, and is as follows:

"Among the great American wonders is the silicified forest, known as Chalcedony Park, situated about 8 miles south of Carrizo, a station on the Atlantic and Pacific Railroad, in Apache County, Arizona. * * * The locality was noticed in 1853 by the Pacific Railroad Exploring Survey. * * * There is every evidence to show that the trees grew beside some inland sea. After falling they became water-logged, and during decomposition the cell structure of the wood was entirely replaced by silica from sandstone in the walls surrounding this great inland sea.

"Over the entire area, trees lie scattered in all conceivable positions and in fragments of all sizes, the broken sections sometimes resembling a pile of cart wheels. * * * A phenomenon perhaps unparalleled and the most remarkable feature of the park is a natural bridge formed by a tree of agatized wood spanning a canyon 45 feet in width. In addition to the span, fully 50 feet of the tree rests on one side, making it visible for a length over 100 feet."

Lieutenant Hegewald writes:

"I rode down the valley to examine the thousands of specimens that lay scattered on each side of the valley along the slopes, which were perhaps 50 feet high; the valley of the Lithodendron, at its widest part, being scarcely a half mile. Along the slopes no vegetation whatever was to be seen, wood being very scarce; the soil was composed of clay and sand mostly, and these petrifactions, broken into millions of pieces, lay scattered all adown these slopes. Some of the large fossil trees were well preserved, though the action of heat and cold had broken most of them in sections from 2 to 20 feet long, and some of these must have been immense trees; measuring the exposed parts of several they varied from 150 to 200 feet in length, and from 2 to 4½ feet in diameter, the centers often containing most beautiful quartz crystals."

Dr. Walter Hough, of the Smithsonian Institution, who has visited the park, writes as follows:

"In the celebrated Petrified Forest, which is some 18 miles from Holbrook, Arizona, on the picturesque Santa Fe Railroad, there are ruins of several ancient Indian villages. These villages are small, in some cases having merely a few houses, but what gives them a peculiar interest is that they were built of logs of beautiful fossil wood. * * * The prehistoric dwellers of the land selected cylinders of uniform size, which were seemingly determined by the carrying strength of a man. It is probable that prehistoric builders never chose more beautiful stones for the construction of their habitations than the trunks of the trees which flourished ages before man appeared on the earth.

"This wood agate also furnished material for stone hammers, arrowheads, and knives, which are often found in ruins hundreds of miles from the forest."

This "wood agate" or "wood opal" is now cut and polished into floor tiling, mantels, clock cases, table tops, paper weights, etc. The silver testimonial to the French sculptor Bartholdi, made by Tiffany & Co., had for its base a section of this wood agate.

Prof. Lester F. Ward, an eminent paleobotanist, who, while officially attached to the staff of the United States Geological Survey, also holds the position of associate curator in the National Museum, expects to visit the Pacific coast this summer, and may return by the southern route. He tells me that if you so desire he would be pleased to visit the region in question for the special purpose of procuring further information regarding the features covered by your inquiry.

In conclusion I would say that all with whom I have consulted are agreed that the "Petrified Forest," or "Chalcedony Park," of Apache County, Arizona, should be preserved as a public park for the benefit of the American people. In no other area is there such a profusion of highly colored stone trees. Fossil wood is scattered over a very great area of Arizona, but the densest portion and chief place of interest is "Chalcedony Park," an area of less than 5 miles square. This region is about 20 miles south of Carrizo station.

A list of papers relating to the Arizona forest trees is appended.

Very respectfully,

RICHARD RATHBUN,
Acting Secretary.

Hon. BINGER HERMANN,
Commissioner General Land Office,
Department of the Interior,
Washington, D. C.

Growing out of the paragraph containing my offer to visit the locality, and a personal call at the General Land Office, the Hon. W. A. Richards, assistant commissioner, in the absence of the Commissioner, wrote me as follows:

DEPARTMENT OF THE INTERIOR, GENERAL LAND OFFICE,
Washington, D. C., August 19, 1899.

DEAR SIR: As requested by you yesterday, I have written a letter to Hon. Charles D. Walcott, Director of the Geological Survey, requesting that you be instructed to visit the "Petrified Forests" of Arizona, in order that you may make report as to the advisability of setting that locality apart as a national park.

I also have had prepared a copy of the memorial of the Arizona legislature relating to the subject, which I inclose herewith.

Very truly, yours,

W. A. RICHARDS.

Prof. LESTER F. WARD,
Paleontologist, Geological Survey.

Owing to your absence in Canada and my departure for the Pacific coast, where I remained during September and October, I did not receive your instructions, above quoted, until the middle of October. At about the same time I received the following letter from the secretary of the Smithsonian Institution:

SMITHSONIAN INSTITUTION,
Washington, October 13, 1899.

The Smithsonian Institution takes pleasure in introducing to its friends Prof. Lester F. Ward, Paleontologist of the United States Geological Survey, and associate curator in the United States National Museum, who visits the region of the fossil forests in Arizona at the instance of the Hon. Binger Hermann, Commissioner of the General Land Office, with a view to obtaining information for the use of the Commissioner in connection with a request of the legislature of Arizona that certain lands in the vicinity of Holbrook, known as the "Petrified Forest," be set aside for a national park.

Any courtesy which may be extended to him will be duly appreciated by the Institution.

S. P. LANGLEY, *Secretary.*

Equipped with these instructions and credentials I left San Francisco on November 1, 1899, and proceeded direct to Arizona. After a week of general investigation in the western part of the Triassic terrane, I arrived at Holbrook on the 9th and entered the special area of the petrified forests on the 10th. I went over the ground with considerable thoroughness and visited about all the localities of interest, taking full notes of the scenic, geologic, and scientific features.

SCENIC FEATURES

With regard to the first of these, viz, the scenic aspect, I can safely say that it has never been exaggerated by any who have attempted to describe this region. The pictures given in the letter of the Assistant Secretary of the Smithsonian Institution, above quoted, are not overdrawn, and the more or less glowing descriptions of Möllhausen, Marcou, Newberry, and other early explorers fall far short of what might be truly said from this point of view. These petrified forests may be properly classed among the natural wonders of America, and every reasonable effort should be made not only to preserve them from destructive influences but also to make their existence and true character known to the people.

Some of the most important considerations that may be urged in favor of the importance of this region with other petrified forests rest upon its geological relations. In the first place, it is much more ancient than the petrified forests of the Yellowstone National Park, of certain parts of Wyoming, and of the Calistoga deposits in California. These latter are of Tertiary age, while the Arizona forests belong far back in Mesozoic time, probably to the Triassic formation. The difference in their antiquity is therefore many millions of years. Scattered blocks of silicified wood do indeed occur in the Trias at other points, but this is the only region in which they are in such abundance as to deserve the name of a petrified forest.

In the second place there is no other petrified forest in which the wood assumes so many varied and interesting forms and colors, and it is these that present the chief attraction for the general public. The state of mineralization in which much of this wood exists almost places them among the gems or precious stones. Not only are chalcedony, opals, and agates found among them, but many approach the condition of jasper and onyx. The degree of hardness attained by them is such that they are said to make an excellent quality of emery.

Perhaps the most prominent of all the scenic features of the region is the well-known Natural Bridge, consisting of a great petrified trunk lying across a canyon and forming a footbridge over which anyone may easily pass. For reasons that will be obvious, the full treatment of this feature is deferred to a more appropriate place.

LOCATION OF THE PETRIFIED FORESTS

It should be understood that petrified or silicified wood occurs in great quantities throughout the Triassic terrane of Arizona, New Mexico, and Utah, and there are hundreds of places where the logs are massed together or even piled one upon another; but the particular region known as the "Petrified Forest of Arizona" lies in the area between the Little Colorado and the Rio Puerco, 15 miles east of their junction, 17 miles east of Holbrook, and 6 miles south of Adamana station on the Santa Fe Pacific Railroad, which measurements terminate at the outer edge of the area on the west and north sides. It is about 8 miles square, and falls chiefly within township 17 north, range 24 east, but extends a short distance on the south into township 16 north, and on the west into range 23 east.

This region consists of the ruins of a former plain having an altitude above sea level of 5,700 to 5,750 feet. This plain has undergone extensive erosion to a maximum depth of nearly 700 feet, and is cut into innumerable ridges, buttes, and small mesas, with valleys, gorges, and gulches between. The strata consist of alternating beds of clays, sandstone shales, and massive sandstones. The clays are purple, white, and blue, the purple predominating, the white and blue forming bands of different thickness between the others, giving to the cliffs a lively and pleasing effect. The sandstones are chiefly of a reddish brown color and closely resemble the brownstone of the Portland and Newark quarries, or the red sandstone of the Seneca quarries on the Potomac River and at Manassas in Virginia, but some are light brown, gray, or whitish in color. The mesas are formed by the resistance of the massive sandstone layers—of which there are several at different horizons—to erosive agencies, and vary in size from mere capstones of small buttes to tables several miles in extent, stretching to the east and to the northwest.

The drainage of the area is to the south, and in the middle of it, having a nearly due southern course, but winding much among the buttes, is the arroyo which has been mistaken for the famous Lithodendron Creek, so named by Lieutenant Whipple in 1853.[1] It is dry most of the year, but has a gravelly bed, often 20 feet in width, and if holes are dug in this gravel to a depth of 4 or 5 feet water will accumulate and stand in them.

The valley of this creek is narrow in the northern and central parts of the area, and there are several short branches or affluents, but at the southern end it broadens out, and its rugged, spurred, and canyoned slopes are highly picturesque. Here is located its principal petrified forest, and this is the region that has been characterized by some as Chalcedony Park. The petrified logs are countless at all horizons and lie in the greatest profusion on the knolls, buttes, and spurs, and in the ravines and gulches, while the ground seems to be everywhere studded with gems, consisting of broken fragments of all shapes and sizes and exhibit-

[1] See Twentieth Annual Report U. S. Geological Survey, Part II, p. 324.

ing all the colors of the rainbow. When we remember that this special area is several square miles in extent some idea can be formed of the enormous quantity of this material that it contains.

Although much fossil wood occurs throughout the whole region, as above delimited, still for several miles to the north of this Chalcedony Park it is less abundant, and it is not until the northern end of the area is reached that another center of accumulation occurs. This lies between two mesas in a valley that opens out upon the general plain which stretches north to the Rio Puerco. It is much smaller in extent than the southern park, but substantially the same general features are presented.

There is still a third center of accumulation, called the "middle forest," which lies some 2 miles southeast of this last and extends to the eastern margin of the general region. It occupies the western slope of the table-land on the east, and is very extensive, stretching a mile or more in a north and south direction and having a width of half a mile in places. It presents many interesting novelties.

GEOLOGICAL CONSIDERATIONS

All the petrified forests thus described are, geologically speaking, entirely out of place, and the trunks bear every evidence of having dropped down to their present position from a higher horizon in which they were originally entombed and from which they have been subsequently washed out. Nor is their original position to be discovered by ascending the several mesas included in the area, although some of these rise 400 feet above the bed of the above-mentioned creek. It is not until the still higher plateau is reached which bounds the whole region and lies more than 700 feet above the valley that the stratum is at last found which actually holds the fossil wood. A geologist might therefore traverse the entire area from north to south, visit all three of the principal forests, and go out with the impression that everything was out of place and with no correct idea of the true source of the fossil wood. Even on the east it would be difficult to settle this question on account of the paucity of the trunks in that direction, but it could doubtless be done by prolonged and careful search. On the west side, however, and directly west of the southernmost area, the plateau is only about 2 miles wide and has a western escarpment with another valley extending both south and west of it. This plateau or elongated mesa is highest on its western side, rising to the 5,750-foot contour line immediately above the escarpment, and here is exposed a fine series of petrified trunks fringing the mesa, with many weathered out on the slope or rolled down into the valley below. A few feet below the actual summit is a bed some 20 feet thick of coarse, gray, conglomeratic, cross-bedded sandstone, at many places in which were found firmly embedded logs and branches of the petrified wood, often projecting from it in the cliffs and clearly in place. This, then, is the true source of the fossil wood, and after several days' study on all sides of the area I became convinced that no other layer holds any of it, at least in this region.

This bed was found at nearly all points where the requisite elevation can be attained, but the petrified logs do not occur in the same abundance throughout. They are massed or collected together in groups or heaps at certain points, and may be altogether absent at others. From their great abundance in the three areas above described, which may be called the upper, lower, and middle forests, respectively, but in all of which they are out of place and lie several hundred feet below their proper position, it must be inferred that the stratum which holds them was especially rich, and the trunks must have lain in heaps upon one another. This bed may have been considerably thicker in these areas than it is farther out on the margins, where it is now found in place.

Only at two points within the general petrified forest area did I find remnants of this bed which had not been broken down and disintegrated. One of these is at the extreme northern end, half a mile northeast of the upper forest. Here there is a small mesa, which lies at an elevation of nearly 5,700 feet, or about 400 feet above the valley which contains the upper forest. It is isolated and its nearly flat top, which is approximately circular, is about half a mile in diameter. The coarse conglomeratic sandstone stratum, 20 to 30 feet in thickness, occupies the summit of this mesa and is often hardened into rock, but in all essential respects it is identical with that of the elongated mesa on the southwest side of the area above described. The petrified wood is less abundant here, but sufficiently common and is embedded in and often projects from the sandstone ledges.

The Natural Bridge.—Besides the fact that this bed lies wholly within the petrified forest area, there is another important circumstance which serves to give it special prominence. One of the most celebrated objects in this entire region is the well-known "Natural Bridge," mentioned by so many travelers and referred to in the documents quoted at the beginning of this report, consisting of a great petrified trunk lying across a canyon and forming a natural footbridge on which men may easily cross. This occurs on the northeast side of the above-mentioned mesa near its rim, and the bed in which it lies is the coarse sandstone which holds all the petrified wood. The Natural Bridge, therefore, possesses the added interest of being in place, which can be said of very few of the other petrified logs of this region.

It was observed in the southwestern exposure and at other points that all the petrified logs and blocks lying in the sandstone or only recently washed out of it are surrounded by a coating of the sandstone firmly cemented to the exterior. The absence of this coating from most of those in the principal forests is due to their long exposure to climatic influences which ultimately disintegrate and detach the sand rock adhering to them and strip them clean to the body of the trunks themselves. That this process requires ages of time is proved by the fact that the Natural Bridge is still coated over a large part of its surface by the remains of the cemented sand rock in which it was once completely imbedded. This is true chiefly of the lower portion, and farther up the trunk it has nearly all disappeared. The trunk is in an excellent state of preservation and is complete to the base, where it is abruptly enlarged and shows the manner in which the roots were attached. This portion still lies partially buried in the sandstone, which is the same in character as that which still adheres to the lower 20 feet. The canyon or gulch has a due north direction and is very precipitous, beginning only 200 yards above the bridge and rapidly broadening in its descent. At the point where the bridge crosses it is about 30 feet wide, but the trunk lies diagonally across and measures 44 feet between the points at which it rests on the sides of the canyon. The angle is nearly 45°, and the tree lies with its roots to the southeast and its top to the northwest. The canyon is here about 20 feet deep, and from its bottom and slopes several small trees are growing, some of which rise considerably above the bridge. The trees are mostly cedars, but there is one cottonwood (*Populus angustifolia*). The root is quite near the brink of the canyon, but rests on a solid ledge for a distance of 4 feet, so that there is no probability that in this dry region it will be endangered by further erosion. The total length exposed is 111 feet, so that more than 60 feet of the upper part lie out on the left bank of the canyon. At about the middle of the canyon, and above where the coating of sandstone still adheres, it measures 10 feet in circumference, giving a diameter of over 3 feet. At the base it is now 4 feet in diameter, but the thickness of the incrustation is not exactly known. At the extreme summit the diameter is reduced to 18 inches. As in the case of practically all the petrified logs of the region, there are no indications of limbs or branches at the top. The significance of this fact will be noted later.

A conspicuous characteristic of all the petrified trunks not only of this area and of the general Triassic terrane of Arizona and New Mexico but of all petrified forests, is their tendency to break across into sections or blocks of greater or less length. All travelers have remarked this, and the sketches given by Möllhausen and in the Pacific Railroad Reports show them thus divided. Some observers have noted the fact that the Natural Bridge has several of these transverse cracks, and all the good photographic views of it show them. I counted four, but most of them seem to be as yet only partial and do not probably extend entirely through the trunk. There is one, however, near the left bank of the canyon which has the appearance of doing so, and the trunk is probably only kept from parting at this point by the mechanical adjustment which causes the adjacent faces to perform the office of a keystone to an arch. Any considerable shrinkage due to climatic or other causes would overcome this influence and the entire bridge would crash to the bottom of the canyon and roll down the escarpment in a number of huge segments.

An examination of the relations of the Natural Bridge to the gulch which it spans shows clearly that the trunk was primarily entombed in the sandstone bed covering this entire region, and that, with the progress of erosion which ultimately carried away the entire plain to the north, as well as in other directions, leaving this small mesa, it was at last exposed, and lay for a great period near the rim of the escarpment. At first it was only partially buried and later came to lie on the surface of the ground. As the land rises somewhat to the south of it, rills were formed above, and in times of floods or heavy rain it obstructed the flow of the water, forming a sort of dam. The water lying against it long after it had ceased to overflow it, tended to disintegrate the rock upon which it lay, until eventually it found its way through beneath it at some one point. The smallest opening of this nature would soon become a free passage for the water, and a simple continuation of this process of local erosion would ultimately result in the formation of the entire gorge as it exists to-day.

The other case which I observed of the presence of the conglomeratic sandstone within the general petrified forest area occurs near its center, about midway between the upper and lower forest along the narrow portion of the valley of the creek described above, on both sides of the canyon and near the level of its bed, at an altitude of about 5,300 feet. The exposure was typical in all respects, and logs were seen projecting from the canyon walls, from one of which specimens were collected. As this exposure is 400 feet below that in which the Natural Bridge occurs and 450 feet below that on the southwestern mesa, its presence there can be accounted for only on one of two hypotheses, either that of the existence of another exactly similar stratum at this horizon or that of a fault, or, what would amount to the same thing, a slide or slipping down of a large block of the uppermost beds in such a manner as not to disturb their stratigraphical arrangement.

The first of these hypotheses is rendered improbable by the fact that a careful study of the beds at the same horizon in other places revealed no such stratum, and it could scarcely be so local as not to be found elsewhere. The second hypothesis seems every way probable, as in such a much-disturbed region it would be easy for the erosive agencies to undermine a small outlier or mesa and cause it to sink down intact to a lower level. The question, however, requires more detailed investigation than I was able to give to it.

Leaving this phenomenon out of the account, therefore, and considering the two exposures, in which there is no question as to their natural position, we may use them as a means of determining whether the strata have any dip, and to some extent in ascertaining the amount and direction of the dip. The topographic map has a 250 feet contour interval, which is too large to be employed with any very great accuracy, and an aneroid can hardly be depended upon for measurements made six hours apart, as had to be done in this case, but as nearly as I could judge from all sources of information the Natural Bridge mesa seems to be between 50 and 100 feet lower than the southwestern mesa. As the distance from the one to the other is about 5 miles, the dip to the northeast is somewhere between 5 and 10 feet to the mile. As, however, the strike was not accurately determined, there is no certainty that this is the true dip of the strata, and more precise observations on a much larger scale will be necessary to settle this question.

Although there is no longer any question as to the true stratigraphical position of these profuse vegetable remains, there are many facts which stand in the way of the supposition that the trees actually grew where we now find them. Several accounts[1] profess that stumps occur erect with their roots in the ground, showing that they grew and were buried and petrified on the spot, but I was unable to confirm any such observations, and on careful inquiry of residents of the country, who had minutely examined every part of the area, I was unable to learn of a single indisputable instance of such an occurrence. The only trunk that I saw standing on end was one that was inverted and had its roots high in air! In fact, from the nature of the case, as I have just shown, there would be no use looking for any such phenomenon in any of the principal fossil forests, since they all lie from 100 to 400 feet below where they were originally deposited. It is only in the beds of coarse sandstone that hold them, therefore, that the evidence need be sought. This I did with the utmost care, but even here I found no example of an upright trunk.

In this, as I was glad to learn after my return on looking the matter up, I was only confirming the observations of Dr. J. S. Newberry, made in 1858 and published in 1861.[2]

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[1] Tagebuch einer Reise vom Mississippi nach den Küsten der Südsee, von Balduin Möllhausen. Leipzig, 1858, p. 300.

Résumé explicatif d'une carte géologique des États-Unis et des provinces anglaises de l'Amérique du Nord, etc., par Jules Marcou. Bulletin de la Société Géologique de France, 2° sér., Vol. XII, 1855, p. 871. Repeated in Geology of North America, etc., Zurich, 1858, p. 13.

[2] Report upon the Colorado River of the West, explored in 1857 and 1858, by

Although it is easy to find petrified limbs and small twigs among the other objects, still these occur sporadically and accidentally at any and all points. They are no more likely to be found beyond the termination of the tall trunks than anywhere else, as would be the case if the trees lay near where they grew. In fact it happened that I never found small twigs in this position, although I searched in hundreds of cases. I found no petrified cones, but I heard vague reports of their having been found. It would be strange if none were preserved in such a vast mass of trunks of cone-bearing trees.

Finally the great abundance of the material would seem to negative the idea that it could have all grown on the same area. Even if every tree had been preserved, there are places where it would have been impossible for them to stand as thickly as they lie on the surface, not to speak of the space that trees in a forest require in order to thrive, as these trees evidently did thrive. And while there is now no place where they lie so thickly in the original bed of sandstone, still, even here they are not only all prostrate but lie in little collections and huddles quite differently from what should be expected if they were precisely where they grew.

The preservation of a forest in situ with the trunks erect could scarcely take place except by some sudden, commonly eruptive agency. Such agencies have undoubtedly operated in the preservation of the petrified forests of the Yellowstone Park and of others that I have visited in Wyoming and elsewhere, in which the stumps and sometimes tall trunks do stand in position with their roots in the ground, but in the region under consideration there are only faint indications of eruptive agencies, certainly not sufficient to account for the phenomena.

The indications therefore all point to some degree of transportation of this material by water antecedent to petrifaction, and the great amount of it at this particular place argues for the existence of such a condition as would arrest the process and cause the

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Lieut. Joseph C. Ives, Washington, 1861, 4°. Part III. Geological Report, by J. S. Newberry, p. 80.

Dr. Newberry's statement is as follows:

"I examined these specimens with some care to determine, if possible, whether they had grown on the spot, as those of Lithodendron Creek are supposed to have done by the members of Captain Whipple's party, or whether they had been transported to their positions. In all that came under my observation, I failed to find any evidence that they had grown in the vicinity. All the trunks are stripped of their branches and exhibit precisely the appearance of those transported to some distance by the agency of water. In confirmation of this view I should also say I found in the marls, with the entire trunks, rounded and water-worn fragments of wood, in some instances silicified and in others converted into lignite.

"I gathered the same impression from all the collections of silicified wood which I observed in this formation in western New Mexico, viz: that all had been *transported*, but not *far* removed from their place of growth."

floating logs to accumulate in masses, as often happens in great eddies or the deltas of rivers. The character of the bed in which they occur further supports this view. The coarse sand and gravel, highly favorable to the process of silicification, denotes the proximity of the land, and the crossbedding bears witness to the existence of rapid and changing currents. As this stratum occupies the highest elevations in this region, the nature of the overlying beds is not revealed, and the question whether the period was followed by one of general subsidence can only be settled by a study of the higher plains lying some distance to the east and north, but it is probable that the bed sank and that finer deposits ultimately buried it at the bottom of the Mesozoic sea, there to remain until the Tertiary epeirogenic movement raised the entire country from 5,000 to 6,000 feet above sea level.

PRESERVATION OF THE PETRIFIED FORESTS

It will be obvious from the above that the Petrified Forests of Arizona constitute an object of interest to all people of culture from both the æsthetic and the scientific points of view, and that the immediate region here considered embraces the most striking features that they anywhere present. As stated in the memorial of the Territorial legislature to Congress, and as confirmed by my inquiries and admitted by all, these natural wonders are attracting thousands of visitors annually, most of whom are drawn there by mere curiosity. This characteristic of human nature, however aimless it may sometimes seem, and however destructive it may often be, forms, under a broader culture, the true foundation of all discovery and progress. It needs encouragement and direction rather than suppression, and the policy should be to increase the attractions and to facilitate access to this as well as other extraordinary natural objects; but at the same time the destructive effects, especially such as tend to reduce the interest, mar the beauty, or lessen the instructiveness of the facts, should be prevented by every proper means.

No one denies that visitors to this region usually carry away with them as much as their means of transportation will permit, but this consists usually, of course, of the smaller objects that lie in such profusion on the ground. At first view it might seem that the immense quantity of such objects makes it impossible that any appreciable impression can ever be made upon the whole mass in this way. This is the same kind of reasoning , or rather unreasoning, that has led to the virtual extinction of the buffalo, and which threatens to exhaust the sources of natural gas; but the class of persons known as "relic hunters" is very large, and the number who will in future visit the Petrified Forests is destined greatly to increase. They usually carry with them some concealed tools or instruments, and with these they are perpetually breaking off pieces of objects of which they wish to carry away souvenirs. In this way the finest trunks are being hacked to pieces and disfigured. For example, there are several places on the Natural Bridge where this process has been going on, until quite large holes or unsightly cavities have been dug in the upper side of the trunk. The small chips and blocks that lie detached on the ground in such quantities vary greatly in form and coloration, and it is, of course, always the most symmetrical and brilliant that are first picked up, and these will eventually be so culled out that only the plainer, unattractive pieces will be left.

It is said that a useful purpose is subserved by sending specimens of the petrified wood to educational institutions. This might no doubt be true under proper regulation, especially if the specimens were duly labeled and authenticated and placed in properly arranged cabinets, to be explained by the teacher. This will scarcely be accomplished by permitting the free access of all parties with the right to carry away specimens at will.

Besides this piecemeal method of making inroads upon the treasures of the Petrified Forests, there are ways in which the work may be and to some extent has already been accomplished on a much larger scale. Many years ago the firm of Drake & Co., of Sioux Falls, undertook the work of manufacturing table tops, mantels, clock cases, pedestals, paper weights, and other articles of furniture and decoration out of these sections of agatized wood, by polishing the smooth surfaces and cutting them into the desired forms. I understand that Tiffany & Co., of New York, obtained through this company the beautiful pieces used by them for such purposes. I visited their house at the time they were engaged in this work, and through the courtesy of Mr. George F. Kunz was shown some of the raw material that they then had in hand, consisting of several sections of immense trunks, of the most brilliant colors. While in the park the present season my teamster informed me that he was employed for a long time hauling these trunks out of the upper forest to Carrizo station. Although, according to all accounts, many carloads of it were shipped to the East, he said that there was a larger quantity left at the station that was not shipped than all that was removed at that time. As scarcely any of this remains at the station now, I asked him what had become of it, and he said it had been carried off little by little by anybody that wanted a piece.

At a later date the Armstrong Abrasive Company, of Denver, conceived the idea of grinding up these trunks to make emery, for which they are said, from their extreme hardness, to be an excellent material. They had a plant for this purpose in Chicago, which they moved to Arizona, and it is now at Adamana station, the nearest point to the Petrified Forests on the Santa Fe Pacific

Railroad. I was informed that the plant never was put into operation, and on inquiring the reason I was told that a Canadian company at about the same time commenced the manufacture of emery and reduced the price to a point below that at which it would be profitable to grind up the Arizona wood. So small a business consideration prevented the somewhat wholesale denudation of the Petrified Forests for commercial purposes. What the next inducement in the same direction may be can not be predicted, but there is always the danger that some powerful mercantile interest may do its destructive work.

LOCAL OPINION RESPECTING THE PROPOSED RESERVE

In making the investigations above recorded, in compliance with your instructions, I endeavored to preserve a wholly neutral attitude on the general question as to the advisability of reserving the Petrified Forests as a national park, and, as a matter of fact, the subject presented many practical difficulties which I was not and am not now able to remove. However clear the general proposition may be that so important a scenic feature and so great a natural wonder ought to be cared for and preserved intact for the enjoyment and instruction of the people, the question as to how this can best be accomplished is somewhat complicated and requires for its solution practical rather than scientific qualities of mind and considerable familiarity with its popular aspects. I therefore considered it my duty to feel, as it were, the local pulse on the subject, and I lost no opportunity to obtain the opinion of leading citizens of Arizona. As a result of my inquiries in this direction I was able to make the generalization that within the Territory the acquaintance with and interest in the project were inversely proportional to the distance of the parties from the region to be affected. In the immediate or close vicinity scarcely any one had heard of the action of the Territorial legislature, and almost no one evinced any interest in the matter. Such was the condition of things in Holbrook and Winslow. At Flagstaff and Williams there was more of both information and interest. I did not go to Prescott nor Phœnix, but I met and heard of people in both these places who were familiar with the movement and deeply interested in its success. First and foremost among these should be mentioned Governor Murphy, whose letter on the subject I give in full below. I chanced to meet his brother, Mr. Frank M. Murphy, of Prescott, on the train while returning from the field, and we had a conversation on the subject, freely discussing all the principal points. He presented a number of cogent arguments in favor of the project, as set forth in the memorial.

At his suggestion, as well as with the advice of several other interested parties whom I met in the Territory, I addressed a letter to Governor Murphy on my return to Washington, asking him for a full expression of his views. In my letter to him I said among other things:

> You will understand that my instructions do not indicate the nature of my report, which is expected to be entirely disinterested upon the question of the advisability of the action asked for by the memorial, but I have not concealed my personal interest in the preservation of this wonderful natural feature of the country, and while my investigations in the immediate locality, which were quite thorough, did not reveal any blameworthy action on the part of any particular individuals, still, with the rapid growth of the Territory and the increasing travel and interest in all such matters in this country, it is obvious that it can not be long before something will need to be done if the scenic features of this region are to be preserved. It is not perfectly clear to me in what way the Government would accomplish this purpose if the reservation were made, and I doubt whether it could be successfully done without the active coöperation of the people of Arizona, and this would have to be something more than could be expected of the local residents of that district.
>
> I therefore write you in the hope that you may freely express your views on the general subject, so that I may avail myself of them in my report. From the geological standpoint I, of course, have all the data necessary for the report, as I spent sixteen days in that general region constantly in the field; but upon the practical question of what the Government should do in case the district is set apart, and especially after such action, I do not feel fully competent to advise and need the assistance of practical public-spirited people, to whom the subject comes nearer home.

He replied promptly to this letter as follows:

> OFFICE OF THE GOVERNOR,
> *Phœnix, Arizona, November 28, 1899.*
>
> DEAR SIR: Your letter of November 23, in reference to the so-called Petrified Forest in northern Arizona, received. I know of no way the Government can preserve this natural curiosity except by having it set aside as a reserve and appointing a keeper to protect it against vandalism.
>
> As you probably observed when there, it is not attractive in the way of natural scenery. It seems to me peculiarly valuable for scientific purposes. I shall be glad to cooperate in any appropriate way for the preservation and protection of the so-called forest, but much expense on the part of the Government in creating a reserve for scenic purposes does not seem to me justified. I do not apprehend that any very large proportion of the agatized material will be removed. Some of the wood has been removed and polished and was exhibited at the World's Fair in Chicago, and I understand an exhibition of it is proposed at Paris next year. I do not think any injury can result from small quantities of the material being used in this way. As the mineral is scattered over the sands in large quantities, some of it covered at considerable depth, and there being a lack of water there for improving the grounds, I do not know of anything that can be done except to guard against its removal, and it is likely that your personal observation will enable you to make the most appropriate recommendations to the Department in regard to the matter.
>
> Yours very truly,
> N. O. MURPHY, *Governor.*
>
> MR. LESTER F. WARD,
> *Paleontologist, United States Geological Survey,*
> *Washington, D. C.*

I also wrote to Mr. Thomas Bunch, of Flagstaff, member of the legislative committee of Arizona, whom I did not meet there, but who, as I learned from a friend of his, Mr. George W. Sturtevant, of Chicago, had taken a special interest in the Petrified Forests. Mr. Bunch also replied promptly and his letter is altogether to the point:

FLAGSTAFF, ARIZONA, *November 28, 1899.*

DEAR SIR: Yours of 23d instant received. I would like to see the Pretrified Forest preserved. If it can be preserved by setting it aside as a national park I hope it may be done at an early date. I have known it and been there often for the past sixteen years, and every time I go I can see the traces of the vandal. Inside of twenty years from now there will be but little left of interest. However, I would be pleased to see some of the best specimens placed in our public institutions, and for that purpose I would permit a limited amount to be removed. I desire to have brought here a nice section and to place it in the Northern Arizona Normal School, and for like purposes I think it advisable to allow the removal. I hope the Government will see that it is not further made the place of amusement with explosives and that no more is blown up with powder.

Yours truly,

THOS. S. BUNCH.
Member Legislative Committee of Arizona.

LESTER F. WARD, Esq.,
United States Geologist, Washington, D. C.

From all this it will be seen that leading citizens and prominent public men in Arizona are sincerely desirous of preserving this interesting spot from vandalism and wanton destruction, and that many of them think that this can best be done by making it a national reserve and appointing the proper guardians to take charge of it. As they show, the expense of this need not be large. A single mounted ranger, such as now patrol the forest reserves of the Colorado plateau, would probably be adequate to this purpose for some time to come.

As nearly all tourists and visitors must approach the Petrified Forests by way of the Santa Fe Pacific Railroad, it is clearly to the interest of that road that they be made as attractive as possible, and there is no doubt that the officers of the road will gladly cooperate with the Government in this matter. A few years ago the nearest railroad station was Carrizo, which is some six miles west of north of the upper forest. The inconvenience of this was apparent to the railroad authorities, and they have recently established a station due north of the forests, only 7 miles from the nearest margin and about 8 miles from the Natural Bridge. This is the station of Adamana, the name being modified from that of the only person living there, Mr. Adam Hanna, upon whom now falls the duty of conducting parties to the Petrified Forests. Mr. Hanna derives considerable revenue from this source, especially as it is usually necessary for parties to stay over night, and he takes care of them. But his house is not convenient to the station and is not adapted for a hotel, and as the number of visitors increases it will become necessary to provide more ample accommodations. There will need to be a hotel with civilized conveniences, and it will eventually be to the interest of the railroad company to provide such, as also suitable conveyances and guides.

The importance of the cooperation of the railroad in this matter struck me so forcibly that I did not consider my mission completed until I had made some inquiries along this line. In returning from the field, therefore, on my arrival at Chicago I called on President E. P. Ripley, of the Santa Fé system, to obtain his views. It chanced that Mr. Frank M. Murphy whom I had met on the train, as above mentioned, was in Mr. Ripley's office at the time I called, and quite a discussion of the general project was entered into. Mr. Ripley expressed a willingness to cooperate with the Government in the matter to the fullest extent in case the proposed action was taken. He was much interested in certain of the scientific aspects of the case that I presented. Mr. Murphy very correctly pointed out that the fact of setting the district apart by the Government as a national park would do more than anything else that could be done to make it known to the public, thus constituting a legitimate advertisement for the road, and working in the joint interest of the company and the people.

RECOMMENDATIONS

After all that has been said it scarcely seems necessary to make specific recommendations. I have endeavored to set forth the facts somewhat fully, and they will doubtless carry with them their own recommendations. But it may not be wholly superfluous, by way of summing up, to specify the suggestions to which the facts seem to give rise in a more succinct form.

1. It seems desirable that the portion of country covered by this report—at least all that falls within the designation of petrified forests—be withdrawn from entry at once, pending further steps in the matter. The amount withdrawn might be considerably larger than that included in the boundaries above specified, in order that no important feature shall be excluded from the tract finally embraced in the park. At present, so far as I could learn, there is only one claim filed on the entire area. This is a claim to a quarter section in the upper forest, filed by Mr. Adam Hanna. At the time that it was proposed to manufacture emery out of the silicified wood several other claims were filed, but, on the failure of the scheme, they were abandoned. Several horse herders have located on the arroyo, or creek, in the center of the lower or principal forest, and have erected a cabin there, but I understand that they have filed no claims and are there merely by sufferance. They are, however, doing no damage to the forests.

2. There is needed a more extended and accurate survey than I was able to make of the proper boundaries of the park. The survey should be made in part by geologists and vegetable paleontologists, in order that due weight be given to scientific considerations—such, for example, as that of causing the park to include points at which the fossil logs are actually in place and good exposures of the rock in which they are embedded.

3. The area fixed upon by this survey should be correctly described and made a public reserve by act of Congress, with proper provisions for its preservation. I need not further specify what I think these provisions should be. Those of other similar acts, with special modifications to suit the case, will probably suffice.

4. As early as possible after the boundaries shall have been fixed there should be made a new topographical survey, which need not, however, be limited to this area, but should include it. The topographic map resulting from this survey should be on a scale of 1 mile to the inch and the contour interval should be 50 feet. The parties making the survey should be instructed to give appropriate names to all the more prominent objects and features, and to locate and name them on the map for the future use of the public visiting the park. A geological map should also be prepared on this new base. The present topographic map, on a scale of 4 miles to the inch, with a contour interval of 250 feet, is very inconvenient, and of little value in the study of this region.

5. No time should be lost in taking measures to prevent the parting and collapse of the natural bridge. As I have shown, this is liable to occur at any time; and although it may last for hundreds of years, still the danger that it may give way, and thus ruin the most important feature of the park, justifies prompt attention. Engineers should at least examine it at once, and if they find it insecure, as stated, they should take steps to strengthen it and render it permanent, which could probably be done at little expense.

As the land on which the natural bridge is located is public land, this work might be done independently of any action in the direction of making it a public reserve, but it seems doubtful whether the authorities could be brought to the point of actually taking action in the matter unless attention be first concentrated upon it by such an act as that of creating a public reservation. But if steps in this direction could be taken in advance of such action, this would diminish the chances of the catastrophe.

I have the honor to be, very respectfully yours,

Lester F. Ward;

Paleontologist, United States Geological Survey.

Hon. Charles D. Walcott,

Director United States Geological Survey.

468. [Appreciation of The Monist and The Open Court]

History.—Written February 4, 1900, on receipt of a printed request. It appeared along with many others in a circular announcement.

It gives me great pleasure to say that I regard *The Open Court* and *The Monist* as the two best American philosophical magazines that we have. *The Open Court* is doing splendid service in acquainting its readers with the true history and character of those religious world-views that are coming from the East and influencing Western thought, but which, in the public mind at large, tend to assume such crude, grotesque, and absurd forms. *The Open Court* is doing much to offset this tendency.

May 15, 1900—Ætat. 58.

469. Review of The Theory of the Leisure Class. An Economic Study in the Evolution of Institutions. By Thorstein Veblen. New York: The Macmillan Co., 1899. Pp. viii + 400, 8vo

History.—Written March 13, to April 6, 1900. When I was at Stanford University in October, 1899, Dr. Ross had a copy of this book and was deeply interested in it. I read enough in it there to gain a correct impression of it, and I resolved to obtain the book on my return, and if possible to meet the author. I reached Chicago on Saturday, November 18, and on Sunday I went out to the University and spent the day with friends. I talked with Dr. Small and Mr. Howerth about the book and made two efforts to see Professor Veblen, but he was away that day. I offered to review the book for the *Journal of Sociology* as soon as I had read it, and Dr. Small promised to send it to me. He told Veblen this, and in the latter's letter of regret at missing my call he expressed his satisfaction that I was to review his book. I did not receive the book till February 7, 1900, but I then immediately commenced systematically reading it, and finished it on March 6. I spent several evenings going through it and taking off salient points for the review.

I first made a rough draft in pencil which I did not complete till April 1, on account of other work in hand, but on April 2, I commenced the final draft and put it through quickly. I sent it to Dr. Small on the 8th, and proof arrived on the 14th. The May number containing it reached me May 15. I had reprints struck off and sent one to Professor Veblen, but Dr. Small had shown him the proofs, and in his letter of April 29, 1900, he says:

> "Your unqualified approval has given me more pleasure than anything that has occurred in connection with the book."

Of other acknowledgments I will only mention that of Professor Gumplowicz. It seemed to touch a responsive chord in him, and he wrote:

> "Sie haben mir durch Übersendung Ihrer Besprechung Veblen's eine grosse Freude bereitet. Ich lese und lese wieder Ihre vortrefflichen Ausführungen. Sie sprechen mir aus der Seele wenn Sie sagen: If there is one thing that the world does not want, it is truth. Bravo!"

If my own estimate were worth anything I should not hesitate to say that of all the reviews of books that I have written this is the best one from nearly every point of view.

The American Journal of Sociology, Chicago, Vol. V, No. 6, May, 1900, pp. 829–837.

August (?), 1900—Ætat. 59.

470. Administrative Report for the year Ending June 30, 1899

History.—Compiled as usual from the monthly reports.

Twentieth Annual Report of the United States Geological Survey, 1898–99, Part I, Washington, 1899 [1900], pp. 65–66.

August 15, 1900—Ætat. 59.

471. Status of the Mesozoic Floras of the United States, First Paper

History.—Written June 10, to August 28, 1899. Withdrawn January 9, 1900, and the results of the field work of 1899 inserted. Submitted in final form January 24, 1900.

While Major Powell was Director of the Geological Survey and I was in charge of the Division of Paleobotany, it became evident to me that I could not cover all the ground. My first work was on the Laramie Group, and later I took up the Potomac Formation. My idea in preparing the correlation papers was to begin with the lowest plant-bearing beds and rise through the whole series. But when Mr. David White, who had worked with Prof. H. S. Wiliams on the Devonian, came, I turned over to him the whole of the Paleozoic. Later, when Dr. F. H. Knowlton had been permanently placed on the staff, I assigned to him the entire Cenozoic flora. This left the Mesozoic as my domain, and as the Laramie was then referred to the Cretaceous, I had always worked on plants of that age. After publishing my papers on the Laramie and the general one on the *Geographical Distribution of Fossil Plants* (see supra, Vol. IV, No. 263), I commenced a study of the Triassic floras and published several papers on them (see supra, Vol. IV, Nos. 301, 312). Then followed my *Potomac Formation* (see supra, Vol. V, No. 405), my paper on *Analogies in the Lower Cretaceous of Europe and America* (supra, Vol. V, No. 421), and my *Cretaceous Formation of the Black Hills as indicated by the Fossil Plants* (supra, No. 463). For the preparation of all these papers I had done a large amount of field work, spending the greater part of nearly every summer for fifteen years in some distant part of the country where plant-bearing beds of one or other of these formations occurred. I now felt prepared to commence a systematic work that should give all the information extant relative to each of the Mesozoic plant-bearing horizons and localities in the country. It was to set forth the present condition, or status, of all the Mesozoic floras of the United States, and these floras were to be taken up in serial order from the lowest upward. The work, therefore, would necessarily begin with the Trias and end with the uppermost Cretaceous. I need not say here that the work was never completed, but the two papers published brought it up to the end of Lower Cretaceous time. I had abundant material for completing it, and planned a third paper on the Middle Cretaceous (Cenomanian, Raritan formation, and equivalents), a fourth on the Dakota Group, Eolignitic, etc., and a fifth on the Laramie, Fort Union, and their equivalents wherever they occur. But it would have taken at least three more years.

Towards the end of 1898 I perfected my plans for beginning this work with the year 1899. A large number of Cretaceous floras, mostly material collected by myself, and some Jurassic and Triassic plants, were either already described, chiefly by Professor Fontaine, to whom I sent them, or were ready to send him as soon as previously sent collections should be reported upon. I could not of course determine these collections myself. Besides the time spent collecting them, there was the laborious task of working up the history and stratigraphy. One department, however, I reserved to myself, and that was the description of the cycads. This was really an immense task, and took all the time I could give the determination of the material.

In my notebook for December 28, 1898, I find this entry:

> "Finished work on the Shasta types, and now have the correspondence, history and MS. of the Jurassic (Oroville, etc.), Shasta, Oregon (Olalla Creek, Riddles, etc.) and Kootanie (Great Falls, Geyser, etc., Montana) all ready to work into my proposed paper on the Status of the Mesozoic Floras of the U. S."

This shows that I had then formulated the title of my "proposed" (to myself only) work. But I began by making a further study of the Trias, the lowest plant-bearing beds being those of the so-called "Newark System."

The amount of editing that I had to do on Professor Fontaine's reports was immense. He had no idea of modern systematic botany nor of the new principles of nomenclature, and the time I had to spend on the synonymy of the older names was very great. In fact, I had to change most of them to conform to the law of priority. All his reports had to be gone through in that way, and made also to conform to the latest principles of classification. The systematic part of the entire work was wholly my own.

The plan was to deal thoroughly with the literature of all the floras and repeat nothing that had been previously published. Only the new material was described as a fresh contribution to the subject in each case. But this was so extensive, chiefly as the result of my own field explorations and collections, that it forms the bulk of the work. In this way I dealt successively, during the first five months of 1899, with the Triassic, divided up into areas, and these further subdivided, including Mr. Atreus Wanner's recent collections from York County, Pennsylvania, and the lost and found classic collection of Dr. Ebenezer Emmons from North Carolina; and with the Jurassic, which had not hitherto been supposed to yield any fossil plants in America. To attempt to give a detailed account of these five months' work would be tedious to the reader, and as the treatment, primarily stratigraphical, is secondarily chronological, he will find sufficient historical details in the text. Suffice it to say that at the end of the fiscal year I had already commenced the systematic preparation of the manuscript of the most considerable contribution that I had thus far made to the publications of the U. S. Geological Survey. The historical and geological parts were for the most part dictated, the several systematic reports were edited in the manner above described and inserted at the proper places in the general arrangement, and the whole finally engrossed in duplicate in a handsome typewritten manuscript.

Hoping to have it finished in time for the Twentieth Annual Report, which was for the year ending June 30, 1899, I forged ahead with it till late in the summer, not taking the field till it was off my hands. This was late in August. Two important subjects had come up, directly connected with the work in hand, which rendered it almost necessary that I visit Wyoming and Oregon that year. These were the Jurassic cycads from the

Freezeout Hills in Wyoming, and the puzzling stratigraphical conditions presented to the geologists who were trying to map certain areas in Douglas County, Oregon, and for the solution of which the expert knowledge of the vegetable paleontologist was demanded. I therefore planned an expedition to these two regions. Everything was done on the monograph by August 25, except to complete the final copying of the manuscript, and I did not quite wait for this to be done, leaving for the field on August 26, when the typist finished her work and forwarded the manuscript on the 28th. I did not much expect to be able to introduce the results of this expedition into this first paper, and was content to leave them for the second paper, in which I proposed to treat the Lower Cretaceous floras, but into which supplementary matter relating to the earlier formations might be inserted. This is what was really done, but I was able, as we shall see, to prepare the reader for the fuller treatment of these floras in that paper.

A third result of this expedition, not specially contemplated by me when I started, was the study I made on my return of the petrified forests of Arizona and of the Triassic beds of the Little Colorado valley in general. What I had said of the Southwestern Area was based on the work of others, I not having ever visited it. Although the reconnaissance I made in November, 1899, was short and unsatisfactory, still it gave me a personal knowledge which was of the highest value. My *Report on the Petrified Forests of Arizona* (supra, No. 467) appeared some time in advance of this paper.

I learned that the manuscript had not yet gone to the printer and I worked up all that I could of these results, embodied them in the duplicate manuscript that I had retained, and then, on January 9, 1900, I withdrew the manuscript and embodied them in it. It was finally submitted on January 24, 1900. A glance at the several chapters will show how considerable and important these additions were. For the Southwestern Area (Arizona) they begin on page 319 and embrace the remainder of that chapter, ending on page 332. The report on the petrified forests is embodied entire, and the account of my excursion down the Little Colorado is prefixed to this. The chapter on the Jurassic flora of Oregon (pp. 368–377) was introduced entire, but the collections, which were sent to Professor Fontaine for determination, were still in his hands, and had to wait for the second paper. As regards the Jurassic cycads from Wyoming, all I could do was to give an account of my visit to the locality and the observations made, together with a geological section that I measured. This occurs on pages 384–388. For further results in all these areas see the Second Paper (infra, No. 519).

Proof of the illustrations reached me on January 27, 1900, and that of the text began to come on March 9, and lasted far into April. As soon as page proofs began to arrive, which was on March 22, I commenced preparing the index from them, which was completed on April 18. My slips were incorporated in the general index of the volume. Both the volume and my reprints appeared on August 13. The latter are immense, and make a large volume by themselves. The index of the volume is stitched in them at the end. Of course such a paper should have been published as a separate monograph, but there was an iron-clad law, or clause of the organic act creating the Geological Survey, which forbade the distribution of the Monographs of the Survey except by sale (at cost plus 10 per cent.) or exchange for works of equal value. This amounted to practical prohibition, because very few would buy such works, and still fewer would send their own especially in exchange. That is not the way scientific writers do. They send their publications to others working in similar lines, and these return theirs in exchange. The consequence was that very few of the members of the Survey were willing to publish in the Monographs. There was no such embargo on the Annual Reports, and they all rushed into them until some of the later years consisted of as many as seven large volumes. It then became so offensive that a new law was passed creating a series of Professional Papers which might be distributed by authors. I was in Europe when the paper appeared and was unable to distribute my reprints till late in October. The responses were the same as in the case of my monograph on the Cretaceous of the Black Hills (see supra, No. 463). They simply stared at it and commented on its size and fine appearance.

Twentieth Annual Report of the United States Geological Survey, 1898–99, Part II, Washington, 1900, pp. 211–748, pls. xxi–clxxix.

November, 1900—Ætat. 59.

472. Elaboration of the Fossil Cycads in the Yale Museum

History.—Written May 13–25, 1900. The paper itself is largely historical and leaves little to be said here. My fourth and last visit to Yale was made May 1–15, 1900, when I determined all the undetermined determinable material. I began this paper while there after the work was about completed, and blocked it out with the collections before me. I submitted it to Dr. Beecher and Mr. Wieland, took it to Washington, and had it copied on the typewriter. I still possess the original pencil draft with numerous notes and descriptions, and the second impression of the final draft. On May 19, I received a letter from Mr. Wieland saying that he had succeeded in matching two or three of the fragments of *Cycadeoidea superba*, which would involve some changes in my article. I was interested in the matter and held back the manuscript till I could investigate it. I was about leaving for Europe, and I went to New York a day earlier than I had intended, viz., on May 24, and the next day I made the journey to New Haven, taking parts of the manuscript with me. The gentlemen in the Yale museum had been busy since I left trying to find pieces that go together, and had found about twenty of them. As I had referred most of these fragments, sometimes doubtfully, to the different species, it was natural to suppose that mistakes in these references would be found. Only one such occurred. A piece that I had referred to the new species, *C. superba*, was found to belong to a large trunk of *C. Minnekahtensis*. Of course these pieces showed only a few of the characters, and the determinations were often based on a certain physiognomic resemblance. It was certainly a good test of the accuracy of my work. I made all the necessary changes in the manuscript and mailed it back to Miss Charlotte H. Schmidt, who completed the final draft and sent it to the *American Journal of Science.*

The article was delayed and did not appear until November 2, or after my return from abroad. Dr. Beecher promised me one hundred extras without charge, which came on the 12th, and there were nearer two hundred. I have never been able to distribute them all, as there are not so many persons in the world who would be interested in the subject. The acknowledgment that I received from Sir Archibald Geikie spoke feelingly of Professor Marsh.

The American Journal of Science, New Haven, Fourth Series, Vol. X [whole No. CLX], No. 59, November, 1900, pp. 327–345, pls. ii–iv.

December 28, 1900—Ætat. 59.

473. The Autochthonous or Allochthonous Origin of the Coal and Coal Plants of Central France

History.—This summary was written out for the Secretaries on December 13, 1900. The excursion referred to, visited Commentry August 29, Decazeville August 31, and Saint-Etienne September 4. Our short stay was inadequate for verifying Fayol's theory at the first two localities, and I was not fully convinced of its truth. At Saint-Etienne, however, there was no such obscurity. On September 6, we visited three localities, one to the southwest of the city, the others to the northeast and north (Eparre, Treuil) where the autochthonous character of the plants was unmistakable, there being fine examples of erect trees and trunks of Calamites, etc., with roots and delicate rootlets penetrating the underclays in such a manner that the least disturbance would have completely altered the effect. I made sketches of some of these in my note-book.

As regards the title of this Communication, I state in my note-book of November 30, that I handed it in as "Autochthony or Allochthony of the Coal and Coal Plants of Central France." I think I changed the wording later. "Autochthonous" is a well-known word, and "autochthonism" and "autochthony" both occur in the dictionaries, but "allochthonous" was of my own coinage on the analogy of the others and ought to be introduced. The "transportation" theory of coal is an old question discussed for a century, yet the geologists have never discovered a convenient adjective.

In presenting my paper, which I did orally from notes, I had a large geological map of France hung on the wall to show just where these places are.

Science, New Series, New York, Vol. XII, No. 313, December 28, 1900, p. 1005.

MR. LESTER F. WARD.—"The Autochthonous or Allochthonous Origin of the Coal and Coal Plants of Central France." Mr. Ward accompanied the excursions of the International Geological Congress to the coal basins of Commentry, Decazeville, and Saint Etienne, and found this to be the principal geological problem presented. M. H. Fayol led the party through the two first-named basins, and lost no opportunity to demonstrate to the excursionists the validity of his well-known theory of deltas, according to which all the materials have been transported from the surrounding country and deposited in small lakes which have been thus gradually filled up. The excursion to St. Etienne was in charge of M. C. Grand'-Eury, whose elaborate treatment of the "Coal Flora of Central France" is familiar to all. He was not less zealous in seeking to make clear the autochthonous origin of the coal plants of that basin. Among the members of the party were Dr. I. C. White, M. H. Potonié and other competent judges of such questions. None of them had any *parti pris*, and all were open to the evidence, which, however, all admitted was in certain respects more or less defective. This was the fault of the conditions, and not at all of the able and courteous expounders of the respective theories. The result at least could not be positively stated in favor of either theory for all the beds, but M. Fayol may be said to have given a correct explanation of the mode of deposition of the Commentry basin and probably of most of that of Decazeville, although in the latter the underclays certainly hold the roots of plants. At St. Etienne M. Grand'Eury showed the party many cases in which the finest fibrils of the roots of erect Calamites were seen to pass across the planes of bedding and penetrate to the underlying conglomerates which formed the original floor; a condition which is wholly incompatible with the theory of transportation or the slightest disturbance of the plants.

January (?), 1901—Ætat. 59.

474. Administrative Report for the year Ending June 30, 1900

History.—Compiled as usual from the monthly reports.

Twenty-first Annual Report of the United States Geological Survey, 1899–1900, Part I, Washington, 1900 [1901], pp. 90–91.

April 12, 1901—Ætat. 59.

475. Review of a Memoir Sur Quelques Microorganismes des Combustibles Fossiles. Par B. Renault. One vol. roy. 8vo, pp. 460, with 66 text figs.; atlas, folio, 30 plates with explanation sheets. Extrait du Bulletin de la Société de l' Industrie Minérale. Troisième série, tome XIII, 4^e livraison, 1899. Tome XIV, 4^e livraison, 1900. Saint-Etienne. 1900

History.—Written February 19–27, 1901. I found Renault engaged on this work when I called on him on August 7, 1900, and he showed me the plates and told me he had named a species for me, of which he let me see his figures. The work reached me February 13, 1901. The review was carefully prepared, not dictated. A rough pencil draft was first made, which was completed February 23, after which it was revised and copied on the typewriter. I have the rough draft and both copies of the manuscript.

Science, New Series, New York, Vol. XIII, No. 328, April 12, 1901, pp. 577–581.

476. La Mécanique Sociale

History.—Written (the French draft) March 5, to May 23, 1900. A rough English draft in pencil was written January 25, to March 4, 1900. Early in January, 1900, I decided to spend a considerable time abroad during that year. The great Paris Exposition was to be held, the International Congress of geologists was to meet in Paris, and the Fourth Congress of the International Institute of Sociology would be held in the Sorbonne. Besides these attractions, there was another which arose later and in which this paper was incidentally involved. This time I proposed to read my own paper before the Congress, and the subject that I chose was social mechanics, upon which I had touched before, but which had now assumed much greater proportions in my mind.

In all my previous French papers I had first made an English draft and then a French draft written on alternate lines, and submitted the latter to a professor of French for correction and idiomatic improvement. I followed this plan in the present case, letting Prof. Jules Maillet give my French paper a thorough revision. I still have this mammoth French draft of 203 pages, which he corrected in red ink, and in glancing it through I am surprised to see how few changes he made. Many that he did make were simply suggestions in the direction of smoothness of diction. Many whole pages are untouched. As I expected to read the paper before a learned body of French-speaking persons, I had several sessions with Professor Maillet in reading it to get his suggestions on pronunciation, accent, carrying over of letters, etc.

The Congress met September 24–27, 1900, in the great chemical amphitheater of the Sorbonne. I had sent on the title of my paper and notified the General Secretary of my intention to be there and read it in person, and it had been duly placed on the program. But I had stated that it was too long to be read entire, and that I should omit large parts of it. The Congress opened on September 24, and I attended the sessions held that day. Prominent members read papers. Dr. De Greef was President of the Institute for that year, but he was prevented by ill-health from attending, and M. Jacques Novicow of Odessa, one of the Vice-presidents, presided. I had read his books, and I met him after adjournment. I was then told that my paper would be the first on the *ordre du jour* for the next day. I was told that the program was very large and it was plainly intimated that I must make my paper short. That evening I wrote out in pencil a preliminary remark relative to the shortening of my paper, which I proposed to read by way of introduction. As this was actually done, and it does not appear in the *Annals*, I introduce it here:

Le mémoire que je vais présenter sur la mécanique sociale a été écrit plutôt pour publication que pour lire. Vous n'auriez pas la patience de l'entendre tout entier, et en même temps il est bien difficile de le condenser, n'étant qu'un résumé d'un grand sujet. Alors, la meilleure manière sera d'omettre certaines parties plus ou moins étendues, et j'ai décidé de supprimer tous les prolégomènes, dans lesquels j'ai essayé de justifier une science de mécanique sociale, ce qui, assurément, n'est pas nécessaire ici.

Je commence, donc, immédiatement avec la classification de la science, et il reste avec vous de dire quand vous en aurez assez.

I made known to the president and secretary before the session opened the nature of this preliminary note, and told them that I should probably stop at the end of the *Statique sociale*. Dr. Worms asked me if I could not read the *Dynamique sociale* first, as likely to be more interesting, and I explained that it would be unintelligible because it follows the other logically.

"I felt inspired and had perfect confidence and a clear sense of the situation. I began right and proceeded to lay my ideas before them in a clear and dignified way that at once riveted attention. I could feel that I had the audience with me. The hall was fairly well filled. It went off splendidly and I put the emphasis always just where it belonged. The *Statique sociale* took about thirty-five minutes, and I proposed to stop. To my great surprise the audience shouted, non! non! and even Novicow insisted that I should go on. So, on I went to the end, and held them perfectly all through. The applause was hearty, and there was some discussion, which had to be continued in the afternoon session."

These words are quoted literally from my diary, written the same evening, and although perhaps a little too enthusiastic, or even egotistic, they describe the event much more faithfully than I could do it now.

The discussion was continued in the afternoon and was opened by M. Kovalewsky, the eminent Russian anthropologist, who, before sitting down, asked me a question, which I was obliged to answer. That was my first attempt at offhand speaking in the French language. After answering Kovalewsky's question, before sitting down I said a few words relative to the letter that the secretary had read from M. Léon Winiarsky, in which he mentioned me and some correspondence which we had had. After the Congress adjourned Dr. Worms asked me to write out and hand him my remarks for publication in the *Annales*, and I did so. See the next head (infra, No. 477).

I received the proof of my paper on February 12, 1901, corrected it, and returned it on the 14th. I had ordered one hundred reprints, which reached me on June 28. They make a fine appearance, but the publishers sought to improve it by repaging it, which to me is highly objectional and wholly unscientific. Long after the proof had been returned, viz., on March 10, I discovered that on page 41 (201 of the volume) I had used the word "sémantisme," when Bréal said "sémantique," and I sent on the correction. Dr. Worms replied on April 10, that he had corrected the error in my reprints, but that it was too late to correct it in the volume, so "sémantisme" still stands there. Another error failed entirely of correction, not being discovered till June 30. This occurs on page 6 (166 of the *Annales*), viz., "*Brobdagnagienne*" for "*Brobdingnagienne.*" I went through all my reprints and corrected it.

Miss E. I. Boshniak, who had translated my *Psychic Factors of Civilization* and *Outlines of Sociology* into Russian, wrote a thirteen-page review of this paper, which appeared in the Moscow magazine called *Russian Thought* for October, 1902, pp. 94–106. In it she set forth all my principles in a scarcely condensed form.

At the close of this Congress, on September 27, the Bureau elected me president of the Institut for the year 1903, in which the next Congress was to be held.

Annales de l'Institut International de Sociologie, Paris, Tome VII, 1901, pp. 163–203; reprint [extrait] with cover and title page, brochure, 8°, Paris, 1901, pp. 1–45.

May, 1901—Ætat. 59.

477. [Discussion of La Mécanique Sociale]

History.—Remarks made September 25, 1900. They were written out the same evening in pencil, and copied in form for publication the next morning. I preserved the rough pencil draft. No proof was submitted to me.

Annales de l'Institut International de Sociologie, Paris, Tome VII, 1901, pp. 234–235.

December, 1901—Ætat. 60.

478. Geology of the Little Colorado Valley

History.—Matter selected October 2–5, 1901, from the manuscript of my large memoir on the *Status of the Mesozoic Floras of the United States, Second Paper*. (See infra, No. 519.) It is a condensation of the chapter on the "Stratigraphical Relations of the Older Mesozoic Deposits of Arizona," pp. 16–28, fig. 8 on p. 45, of that work. All necessary details will be found under the history of that memoir.

The American Journal of Science, New Haven, Fourth Series, Vol. XII [whole No. CLXII], No. 72, December, 1901, pp. 401–413.

January (?), 1902—Ætat. 60.

479. Administrative Report for the year Ending June 30, 1901

History.—Compiled July 3, 1901, from the monthly reports in the customary manner.

Twenty-second Annual Report of the United States Geological Survey, 1900–01, Part I, Washington, 1901, pp. 101–103.

480. Contemporary Sociology

History.—Written July 19, to September 1, 1901. Revised and many references added November 11, to 19, 1901. This memoir is a reject or eject, from my *Pure Sociology* that I began to write on July 14, 1901 (see infra, No. 487). My original plan was to devote one good long section near the beginning to the various systems of sociology, before proceeding to set forth my own system. This was to form the second and much the longer section of the second chapter on "The Establishment of the Science." I wrote the whole of it with that intention, and only changed my mind in November after I had written four chapters, and began to see how large the book was going to be. I then determined to do something to reduce its bulk, and I said to myself, I will devote the entire book to my own system, and let that long section on other systems go elsewhere. It was then (November 3) that I wrote to Dr. Small and said to him, incidentally when speaking of other matters, that I had a large treatise that I had had to crowd out of a book I was writing, which I could send him in instalments, if he wished, for the *American Journal of Sociology*. He replied November 7, asking me to send the matter on, as he hoped to publish it in the number for January, 1902. But of course he was counting without his host, and I had to write again and tell him how big it was. His reply of the 13th began with these words:

> "You can't scare me that way! We can take care of 30,000 words and still cry for more. There are a few people still in the United States and the rest of the world who want sociology straight and know it when they see it. I will get twenty pages or so into the January number unless my present calculation is at fault."

At the time this correspondence took place I had not verified all the numerous references and quotations used, many of which I had taken at second hand, and I then proceeded to do this, which required a number of evenings spent in the Library of Congress and some correspondence. I was resolved to see the originals of everything. This has always been my method. It was November 20, before I felt quite ready, and then there still remained a few outstanding matters that I worked into the proofs. On that day I sent the entire manuscript in an express package to Dr. Small. I made some suggestions as to the subdivision of it, but left that chiefly to him. It was not till November 18, that I decided to call the whole series *Contemporary Sociology* instead of *Systems of Sociology*, as I had written it in the original, and as what is left of it still stands in the book. On November 19, I prepared a table of contents, but this could not well be used.

It all worked smoothly, the three papers appearing in the consecutive numbers of January, March, and May, 1902, which reached me respectively on January 24, March 27, and May 24. I had suggested to Dr. Small that it would be best not to strike off extras of each paper separately, but to wait till all were out and then stitch the three articles together into a single brochure. This was done, and my reprints in this form arrived on June 5. I ordered one hundred, but only eighty-five came, and I distributed them as judiciously as I could. It is remarkable what a demand there has been for this paper. Most of the applicants suppose it to be a book. My stock was soon exhausted, but I am still receiving inquiries with regard to it. I can only refer the parties to the numbers of the *Journal* in which the several parts appeared.

The paper gave rise to considerable correspondence, and I received many congratulatory letters. I shall not speak of these here, but will mention Senator Lilienfield's letters, because they contain criticisms some of which are probably in the main just, especially of my statement that he agreed with Barth that sociology was virtually the philosophy of history, which he said he did not teach, but quite the contrary. I received a long letter from him dated February 6–19, 1902, *i. e.*, after reading the first paper when it appeared in the January *Journal*. It is in that, that I treat the organic theory, and his criticism is directed to that.

But it is simply a defense of his own peculiar way of regarding that theory and contains little that is new. His other long letter is dated July 4–17, 1902, after receiving the reprint. It relates chiefly to the above-mentioned statement on p. 630 relative to the philosophy of history.

There was quite an extended review of this paper in *La Lectura. Revista de Ciencias y de Artes*, for September, 1902, from the pen of Prof. Adolfo Posada, who has done so much since to make my ideas known. Of course it is highly appreciative.

On July 23, 1902, I received a letter from Eckart Schumacher, representing the Wagner'sche Universitäts-Buchhandlung of Innsbruck, requesting my permission to publish a German translation of my *Contemporary Sociology*, and saying that Dr. Gumplowicz had found a competent translator. I replied on the 25th, giving my permission and sending a copy of the reprint. On August 20, I received a letter from Dr. Gumplowicz, explaining the circumstances. He had read the pamphlet with the greatest interest and he knew a young man, one of his former students, Dr. Richard Thurnwald, who had also read and admired it and offered to translate it into German. He had sent it to him and also my *Sociology as a Science* (see infra, No. 485), and thought perhaps he might translate that too and publish both together. But he explained that there would be delays on account of other work on which Dr. Thurnwald was then engaged. On April 3, 1903, he wrote me that the translation of the *Contemporary Sociology* was completed, and it seems to have been sent to the Innsbruck publishers near that time with the "Geleitwort des Übersetzers," but as the other essay was to be included with it, the publishers waited for it to arrive before going ahead with the composition. This explanation was given me by Dr. Gumplowicz in a letter dated May 21, 1903.

I sailed for Europe on June 6, 1903, and had my first and memorable interview with Gumplowicz at Gratz on August 23. The subject of the translation was mentioned, and supposing that Thurnwald was in Berlin, where I was to go next, I bethought myself the next day, after my return to Vienna, that I might meet him there if I knew his address. I therefore wrote to Gumplowicz and asked him to send it to me. But he knew that Thurnwald was in London, and as I had given him my Berlin address, he wrote to Thurnwald and asked him to write to me in Berlin, which he did, regretting not being in Berlin and hoping to see me in London, which proved impossible. I mention this to show that all was agreeable at that date. On January 21, 1904, I received a letter from Gumplowicz, written on the blank portion of one to him from Schumacher, in which latter it was stated that Thurnwald, who was then in Berlin, had not returned the last proofs, and could not be induced to allow his name to appear as the translator of the paper. He had written the publishers a strange and almost uncivil letter. Gumplowicz was very much distressed over it, and feared that something was affecting the mind of his former pupil. But, as he said, it made little difference whether the translator's name appeared or not, and the work was pushed through and soon came out in its present form.* I received three copies of it on February 29, and ordered twenty more, which arrived on March 15. I sent a copy to Dr. Small, and in a letter dated March 24, 1904, he says:

> "Your *Contemporary Sociology* looks a little strange under its German title, but I am delighted that the Germans are beginning to catch on. I had an opportunity to use the fact of this translation to point a moral with apparent good effect in talking with the five German professors who have been visiting this week. They were not at all slow to acknowledge that Americans must be counted with in an increasing number of the departments of knowledge."

This work has also been translated into Japanese. This was done by Mr. Unoské Wakamiya of Gugakuji, Kamakura, Japan, and was published in the *Tokyo Economist* some time in 1908. I have not seen the numbers, and do not know the exact date. I have the information from Mr. Wakamiya himself in a letter dated May 7, 1909.

The American Journal of Sociology, Chicago, Vol. VII, No. 4, January, 1902, pp. 475–500; No. 5, March, 1902, pp. 629–658; No. 6, May, 1902, pp. 749–762; covered brochure consisting of these articles stitched together, Chicago, 1902.

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* It is a neat octavo brochure of 84 pages with cover and title page, both bearing the vignette of the publishing house and the following words: L. F. Ward | Soziologie von Heute| (Übersetzung aus dem Englischen) | (vignette) | Innsbruck | Verlag der Wagner'schen Universitās-Buchhandlung. | 1904.|

481. Sociology at the Paris Exposition of 1900

History.—Written November 24, 1900, to April 16, 1901. I made up my mind to go to Europe in the summer of 1900, more than a year in advance. I wanted to do a good deal of library work in connection with the bibliography of fossil plants, and I wanted to visit museums in Europe to examine types that I was dealing with. I also hoped to make some geological excursions and visit some important plant-bearing beds that I did not see in 1894. But the great Paris Exposition was also a prime attraction, and I did not know at first but that I might obtain a jurorship in some department, but notwithstanding the strenuous efforts to this end made by my friend, Mr. Victor Louis Mason, who had great influence with "the powers that be," and for which I take this my first opportunity to thank him publicly, all measures looking to this end failed. Mr. Peck seemed to know all his men in advance, and recommendations, vouchers, and qualifications, it was learned, had no weight with him.

On June 1, 1899, I chanced to be in the office of Dr. Wm. T. Harris, Commissioner of Education, where calls from me were always welcome, and this question of jurorship came up. He had much to do with the educational exhibits and wanted me to serve on one of the committees on that subject. He volunteered to write to both Peck and Rogers. It was too early yet, but he asked me to jog his memory by telephone when the proper time came. He then told me that he had a little fund out of which to pay some one to write up the sociological aspects of the Exposition, and said he had been trying to think of the right man to do it. He said that if I would undertake it he would not look any farther. As I intended to interest myself in educational matters there, I told him that I thought I could do it. There the matter rested until April 30, 1900, when, calling again, as I had done many times in the interval, I asked him if he remembered the previous interview. He said he did, and had done nothing further in the matter. He said it was time to act, and he made me wait for him to dictate a letter of introduction, and promised to send me full instructions in a few days. He must have dictated them the same day after I went away or the next day, for they bear date May 1, 1900. The letter of introduction is as follows:

DEPARTMENT OF THE INTERIOR,
BUREAU OF EDUCATION,
WASHINGTON, D. C.
April 30, 1900.

This will introduce Professor Lester F. Ward who visits the Paris Exposition and the Congresses in connection with it to observe the status of sociology not only as it will appear in the papers of the Sociological Congress but also in any of its applications to education as shown by papers in other Congresses.

Professor Ward, who has prepared noteworthy books on the subject of sociology, and given lectures at higher institutions of education in this country on the subject, has promised to report this phase of the Paris Exposition and Congresses for this Bureau.

Any courtesies that may be shown Professor Ward in the way of facilitating his investigations will be gratefully appreciated by him and reciprocated by this Bureau when occasion offers.

W. T. HARRIS,
Commissioner of Education of the United States.

The following was my letter of instructions:

DEPARTMENT OF THE INTERIOR,
BUREAU OF EDUCATION,
WASHINGTON, D. C.
May 1, 1900.

Prof. Lester F. Ward, 1321 M Street, N. W.,
Washington, D. C.

MY DEAR PROFESSOR WARD:

According to my understanding of the engagement which we talked over yesterday, you will prepare for my Annual Report a chapter of not less than twenty-five thousand words on the subject of sociology or social science as presented not only at the Congress entirely devoted to that subject, but also as it appears in its relation to education and other subjects in special papers offered at the other Congresses. In other words, I should like to have your impressions (and data on which they are based) as to the question of sociology, what progress it is making in Europe and what aid it is lending to the other sciences in solving their problems. The honorarium promised is two hundred and fifty dollars.

Hoping that you will have a prosperous journey abroad,

Very sincerely yours,

W. T. HARRIS,

Commissioner.

I sailed on May 26, 1900, but was more than a month in England, and did not reach Paris till July 19. I remained there, with the exception of several geological excursions and a brief visit to the Touraine, till September 30. I identified myself with the Anglo-American group of the International Association for the Advancement of Science, Arts, and Education, before which, as mentioned in my report (p. 1457), I delivered a lecture August 17, on the *Dependence of Social Science upon Physical Science.* I had taken steps before leaving America to have myself regularly enrolled as a member of the Congress for Instruction in the Social Sciences, and I attended that regularly and took some part in the proceedings, was chosen to preside over the Congress for August 1, and addressed the Congress on August 2, when I took occasion to express my views on the position of sociology in the curriculum, much as I had done in the *Outlines of Sociology* (p. 136). But the idea prevailed in the Congress that there were forms of social science that could be taught even in primary schools. That Congress lasted from July 30, to August 3. The other important educational Congress, called the International Congress of Social Education was held September 26–29, which conflicted with the Congress of the International Institute of Sociology, which I was obliged to attend, and I could not be present at both. I learned all I could of it and gave the account on pages 1564 to 1571 of my report. The last Congress to meet, and the one that interested me most, was of course that of the International Institute of Sociology. I have given the history of the Institute itself quite fully in my report (pp. 1571–1575) and especially that of the Congress of 1900 (pp. 1575–1579), and I concluded the report with the English version of the paper that I read before that Congress (pp. 1579–1593).

It remains to give the history of the writing and publication of this report. I reached Washington on my return from Europe on October 22, and on November 12, I was ready to commence work on the report for the Bureau of Education. I had obtained most of the documents relating to the several Congresses, but a few were still wanting and some time was spent in finding them and getting them together. I also had all the reports made to the Congress for Instruction in the Social Sciences. The writers of these reports had been selected a year in advance of the meeting of the Congress and arranged with to have their reports in the hands of the proper officers long enough before that meeting for the reports written in other languages to be translated into French and printed. In fact nearly all were ready for distribution at the opening session on July 30. Being all in French, their translation into English constituted the heaviest part of my task. On November 24, I was ready to commence work, but seeing that my material would exceed the 25,000 words mentioned in my instructions, I called on Dr. Harris on that day and discussed the whole subject with him. I told him that I considered these reports as of the greatest importance, and that it would be a great thing to have them all published in his annual report. He fully concurred in this and dictated a supplementary letter of instructions, the important part of which was as follows:

> "My former letter named as a minimum 25,000 words and it was understood that you might put in documents relative to the subject, and otherwise expand the article, if you chose, up to 100,000 words or less."

That same evening I broke ground on the report. The title that I then gave it was: *Status of Sociology as a Branch of Education at the Close of the Nineteenth Century*. By the 29th I had written the introductory matter and was ready to commence translating the French reports. The reports of the Englishmen I hesitated to translate into English, presuming that they were originally written in their own language. This, in fact, was the case, and even some verses that they contained had been translated, usually very badly. I therefore wrote letters to Messrs. Sadler, Aves, and Webb, asking them whether the originals of their reports could be obtained. Some were in their own hands and they sent them promptly, others were still in Paris, and I had to correspond with the Secretary of the Congress, M. Hauser, who sent me those of Messrs. Sadler, Webb, Abney, Cobden-Sanderson, and Plunkett. He had returned that of Mr. Aves. I wrote again to him and he sent it. It arrived on April

11. I also wrote to the two Americans, Hill and Thurston, and obtained the originals of their reports. With the exception of a few outstanding ones this work was completed by the end of January, 1901, and they were all in by April 11, that of Mr. Aves being the last. The other Congress (Social Education) was written up February 9–15, and the International Institute of Sociology February 22–26. On the 28th I commenced making a copy for this report of the rough English draft of my paper on *Social Mechanics*. (For the history of this paper see supra, No. 476.) This was completed on March 11. I then prepared the table of contents and drew up the title page on the 12th. Previous to this I had decided to shorten the title and call the report: *Sociology at the Paris Exposition of 1900*. I then had to wait nearly a month for answers to my letters to Europe and for the originals of the English reports, which came, as above stated, and the work was closed on April 16, and submitted on the 17th.

On June 13, 1901, when I went after my mail at Holbrook, Arizona, which I reached late the night before, I found the galley proof of the whole of this report, awaiting my return from the interior of the country. I was not through with my geological work and could not return immediately to Washington. I therefore decided to suspend the latter and devote myself to the correction of the proofs. It took four days, and on the 16th I returned it to the Bureau of Education. I reached Washington on the 28th. Page proof had arrived and I proceeded to correct it. This was completed on July 1. A full set of the second revise was loaned me on September 11, but was never called for, and I am using it as the copy for this work. The volume containing the report appeared on March 4, 1902, and on the 6th and 7th I received 150 copies of the chapter (XXVIII), called "Advance Sheets," as a reprint with a cover, and franked envelopes for mailing them. The *Annual Report* for that year (1899–1900) was immense and had to be bound in two large volumes, but the pagination is continuous through both volumes. This chapter occurs in the second volume. I distributed the reprints freely, sending one to each of the authors of the reports to the Congress. The replies were all pleasant, but few had had time to do more than look it through. My paper on *Social Mechanics*, with which it concludes, is practically buried there, many to whom I sent it not knowing of its existence. Of the letters that came, I will quote one which seems, in the light of subsequent events, almost to cast a shadow before:

BROWN UNIVERSITY, PROVIDENCE,
March 17, 1902.

MY DEAR MR. WARD:

I thank you for the copy of *Sociology at the Paris Exposition of 1900.* I am very glad you were entrusted with the making of this valuable report.

I am looking forward with pleasure to the appearance of your work on Sociology which you mentioned when I saw you in Washington.

Whenever you come this way I hope you may stop over in Providence.

Sincerely yours,
GEO. G. WILSON.

Report of the Commissioner of Education for the year 1899–1900, Washington, 1901, Vol. II, Chapter XXVIII, pp. 1451–1593 [Vol. II not issued till March, 1902]; Advance Sheets. United States Bureau of Education. Chapter from the Report of the Commissioner of Education for 1899–1900. Chapter XXVIII. Sociology at the Paris Exposition of 1900. Washington: Government Printing Office. 1901 [1902]. (Covered pamphlet.) Pp. 1451–1593.

April 11, 1902.—Ætat. 60.

482. [Definition of Sociology]

History.—Written March 15, 1902. This is the abstract furnished Mr. Wead of the paper read the same evening (see infra, No. 485), which, with slight changes, appeared in both *Science*, and the Bulletin of the Society.

Bulletin of the Philosophical Society of Washington, Vol. XIV, 1900–1904, Washington, 1906, p. 393; Science, New Series, New York, Vol. XV, No. 380, April 11, 1902, p. 588.

483. The Word "Ecology"

History—Written March 30, 1902. There were several other replies to the note of Mr. Horace White, Editor of the *New York Evening Post* in *Science* of March 28, 1902 (Vol. XV, p. 511), all to the same effect. Several pointed out that "œcology" was first used by Haeckel in 1866 in his *Generelle Morphologie* (*Ökologie*), which I also knew. If I had done the botany for the *Century Dictionary* from A to G (as I did from H to Z) "ecology" would have gone in. Of course I saw that it went into the *Supplement*, but that did not appear till 1909. It is also in the *Supplement* to the *Standard Dictionary*, but that is dated July, 1903. The *Supplement* to the *Webster*, however, had been out several years before White wrote his note. The proper definition of the word "ecology," or "œcology," should be: the science of the relations of organisms to their environments. This was the sense in which Haeckel originally used it.

Science, N. S., New York, Vol. XV, No. 380, April 11, 1902, p. 593.

It is stated by a correspondent in a recent issue of *Science* that the word "ecology" is not in the dictionaries. The word "œcology" will, however, be found, so spelled in accordance with its etymology. It is only after words become universally known that these diphthongs are dropped, *e. g.*, "paleontology," but we still have "æsthetics," "archæology," etc. I took pains, however, to have "ecology" put in the *Supplement* to Webster; with a cross reference to "œcology."

June, 1902—Ætat. 60.

484. [Le Matérialisme Historique]

History.—Remarks made September 27, 1900, at the Fourth Congress of the International Institute of Sociology (Paris Exposition). Written out September 27–28, 1900. The subject of historical materialism was the *ordre du jour* for September 26 and 27. I was catechized a good deal in the course of the discussion and had to make several short speeches on the 27th. I put them all into one paper and gave them to Dr. Worms. The proceedings of this Congress were too voluminous for a single volume, and this part had to go over into a second which appeared a year later.

Annales de l'Institut International de Sociologie, Paris, Tome VIII, 1902, pp. 185–186.

June 19, 1902—Ætat. 60.

485. Sociology as a Science

History.—Written April 8–10, 1902. On January 29, 1902, Mr. Charles K. Wead, Secretary of the Philosophical Society of Washington, called on me and asked me if I would be willing to take part in a discussion that was to take place some time later of the definition of certain sciences, and give a definition of sociology, or present its claims to be regarded as a science. I had resigned my membership at the end of 1899, because, after all the other scientific societies of Washington had been founded, the

Philosophical Society, which used to cover them all, had become simply a physical and mathematical society. If I read a paper before that society, therefore, it would be by invitation, and this was my invitation. I consented, and he promised to inform me when the subject would come up. On March 7, I received a letter from Mr. Wead saying that the meeting would take place on March 15, with the following program:

On the foundation of some modern sciences:

Introductory	Dr. Dall
Statistics	Col. Wright
Economics	Prof. Falkner
Psychology	Prof. Pace
Sociology	Dr. Ward

All except Dr. Dall were invited, not being members of the Society. This program does not differ from the final printed one that soon came (March 12), except that in the latter the word "definition" is used in place of "foundation." He asked me to prepare an abstract of one hundred or two hundred words to be used in his report as Secretary regularly furnished to *Science*, and I did so (see supra, No. 482). All the preparation I made was to select on the day before the meeting some extracts from Chapter IV of the manuscript of *Pure Sociology*, sufficient to occupy the fifteen minutes allotted to each speaker. I did not care to hear the other speakers, being very busy, and did not go to the meeting till after 9 o'clock. Professor Pace was reading his paper when I arrived and after he was through I was called and read mine. It made a very strong impression. There was, I think, no time for discussion, but a number congratulated me after the adjournment on having fairly established the right of sociology to a place among the exact sciences.

On March 24, I received a note from Dr. Cattell asking me to contribute my remarks for publication in the *Popular Science Monthly*, which he was then editing, to which I consented, and as soon as I could get round to it I wrote them out in the form in which they appeared. The reader will see that most of the article is contained in the above-mentioned chapter. I received proof on May 12, and the June number was in the book stalls on June 9. On June 17, I received reprints of all the papers of the symposium together with a cover in which the whole pamphlet was credited to Dr. Dall! All I could do was to cut my part out and distribute it as an excerpt. I had written my paper as an independent article and called it *Sociology as a Science*, and it retained that title in the proof that I received, a duplicate of which I still have, but in the final make-up the whole series was entitled: *On the Definition of some Modern Sciences*, and each article simply bore the name of the science treated. The title of mine was therefore simply *Sociolgy*. This was very unsatisfactory. It is almost meaningless when the paper is considered by itself. I shall therefore in this work adhere to my original title.

This article, appearing as it did, eight months in advance of *Pure Sociology*, fixes the earlier date of some of the most important principles set forth in that work. I draw attention specially to the canon by which the complex sciences may be relegated to the list of exact sciences (p. 113 of the article; p. 48 of *Pure Sociology*) which I had formulated two years before but of which I here give two formulas where only one had previously been given. Some eminent sociologists wrote me that they had long felt that there was some such law, but that they had been unable to formulate it. *Public Opinion* reproduced considerably more than half of my article, and much later it was published entire in the *Plebs Magazine*.

Popular Science Monthly, New York, Vol. LXI, No. 2, June, 1902, pp. 113–119; Public Opinion, New York, Vol. XXXII, No. 23, June 5, 1902, p. 718; The Plebs Magazine, Oxford, Vol. II, No. 4, May, 1910, pp. 81–87.

SO far as the definition of sociology is concerned, it is simply the science of society, or the science of social phenomena. All the more specific definitions that have been proposed have created more confusion than they have cleared up. What is needed in sociology is not definitions, but a clear presentation of the scientific principles underlying it. There is one such principle, failure to recognize which causes most of the difficulty in endeavoring to establish the science. This principle, however, is one that also applies to psychology, biology, and all the other sciences classed as 'complex'—to the biological sciences and the moral sciences. This principle may be formulated as follows:

In the complex sciences the quality of exactness is only perceptible in their higher generalizations.

Or, since exactness, *i. e.*, uniformity, invariableness, reliability, etc., is what constitutes scientific law, the same truth may be simplified and reduced to the following form:

Scientific laws increase in generality as the sciences to which they apply increase in complexity.

In sociology, therefore, which is the most complex of the sciences, the laws must be the most highly generalized. I shall not attempt to do more here than bring forward a few illustrations of the above propositions. It is clear that the method of sociology is essentially that of generalization, *i. e.*, of grouping phenomena and using the groups as units. Nature works by this method, for example, in chemistry, where it is believed that the higher compounds have as their units compounds of lower orders.

Social phenomena are obtrusive, ever-present, multitudinous. Their very proximity is a bar to their full comprehension. This I have called 'the illusion of the near,' and likened it to trying to see a city or a forest while in its midst. The near is indefinite and unsymmetrical. It presents a multitude of dissimilar, heterogeneous objects. They appear to be without order. To see order in them they must be seen from a distance. Beauty is almost a synonym of order. A landscape is beautiful because distance has reduced its chaos of details into order. A mountain seen at a distance is a symmetrical object of rare beauty, but when one is climbing it the rocks and crags, the ridges and gulches, the trees and prostrate logs, the brush and briars, constitute a disordered mass to which the term beauty does not apply. It is much the same with social phenomena. They must be seen, as it were, at long range, which brings groups of facts into relief and shows their relations.

What Dr. Edward B. Tylor has called 'ethnographic parallels,' viz., the occurrence of the same or similar customs, practices, ceremonies, arts, beliefs, and even games, symbols and patterns, in peoples of nearly the same culture at widely separated regions of the globe, proves, except in a few cases of known derivation through migration, that there is a uniform law in the psychic and social development of mankind at all times and under all circumstances working the same results. The details will vary with the climate and physical conditions, but if we continue to rise in the process of generalization we shall ultimately reach a plane on which all mankind are alike.

Even in civilized races, there are certain things absolutely common to all. The great primary wants are everywhere the same, and they are supplied in substantially the same way the world over. Forms of government seem greatly to differ, but all governments aim to attain the same end. Political parties are bitterly opposed, but there is much more on which all agree than on which they differ. Creeds, cults and sects multiply and seem to present the utmost heterogeneity, but there is a common basis even of belief, and on certain occasions all may and sometimes do unite in a common cause.

Not only are the common wants of men the same, but their passions are also the same, and those acts growing out of them which are regarded as destructive of the social order and are condemned by law and public opinion, are committed in the face of these restraining influences with astonishing regularity. This is not seen by the ordinary observer, and every crime or breach of order is commonly looked upon as exceptional. But when accurate statistics are brought to bear upon this class of social phenomena they prove to be quite as uniform, though not quite so frequent, as the normal operations of life.

The ordinary events of life go unnoticed, but there are certain events that are popularly regarded as extraordinary, notwithstanding the fact that the newspapers everyday devote more than half their space to them. One would suppose that people would sometime learn that fires, and railroad accidents, and mine disasters, and boiler explosions, and robberies, and defalcations, and murders, and the whole list of events that make up the daily news, were normal social phenomena. Nearly every one of them has occurred nearly every day in nearly every country in the world during the lives of us all and those of our fathers and grandfathers. But this enormous mass of evidence has no effect whatever in dispelling the popular illusion that such events are extraordinary. There is nothing new in 'news' except a difference in the names. The events are always the same. All this applies equally to those larger events that make up the bulk of what is popularly understood as human history. Viewed from the standpoint of sociology, history contains nothing new. It is the continual repetition of the same thing under different names. This is what is meant by generalization. We have only to carry it far enough in order to arrive at unity. Society is a domain of law and sociology is an abstract science in the sense that it does not attend to details except as aids in arriving at the law that underlies them all.

We may call this the *sociological perspective*. It is the discovery of law in history, whether it be the history of the past or the present, and including under history social as well as political phenomena. There is nothing very new in this. It is really the oldest of all sociological conceptions. The earliest gropings after a social science consisted in a recognition of law in human affairs. The so-called precursors of sociology have been those who have perceived more or less distinctly a method or order in human events. All who have done this, however dimly, have been set down as the heralds of the new science. Such adumbrations of the idea of law in society were frequent in antiquity. They are to be found in the sayings of Socrates and the writings of Aristotle. Lucretius sparkles with them. In medieval times they were more rare, and we scarcely find them in St. Augustine, but Ibn Khaldoun, a Saracen of Tunis, in the fourteenth century gave clear expression to this conception. His work, however, was lost sight of until recently, and Vico, who wrote at the close of the seventeenth century and beginning of the eighteenth, was long regarded as the true forerunner of Montesquieu. Still, there were many others both before and after Vico, and passages have been found reflecting this general truth in the writings of Machiavelli, Bruno, Campanella, Bacon, Hobbes, Locke, Hume, Adam Smith, Ferguson, Fontenelle, Buffon, Turgot, Condorcet, Leibnitz, Kant, Oken, etc.

The theologically inclined, when this truth was brought home to them, characterized it by the phrase "God in history," and saw in the order of events the divine hand guiding the acts of men toward some predestined goal. This is perhaps the most common view to-day outside of science. But science deals with phenomena. Sociology therefore can only become a science when human events are recognized as phenomena. When we say that they are due to the *actions* of men, there lurks in the word 'actions' the ghost of the old doctrine of free will, which, in its primitive form, asserts that any one may either perform a given action or not according as he may will. From this point of view it is not supposed that any event in human history need have occurred. If the men whose actions caused it had willed otherwise it would not have occurred.

The scientific view of history is that human events are phenomena of the same general character as other natural phenomena, only more complex and difficult to study on account of the subtle psychic causes that so largely produce them. It has been seen more or less clearly by the men I have named and by many others that there must be causes, and the philosophy of history that gradually emerged from the chaos of existing history was simply an attempt to ascertain some of these causes and to show how they produced the effects. To those who make the philosophy of history coextensive with sociology this is all that sociology implies. Certainly it was the first and most essential step in the direction of establishing a science of society. The tendency at first was strong to discover in the environment the chief cause of social variation, and some authors sought to expand the term *climate* to include all this. This doctrine was of course carried too far, as shown by the saying that "mountains make freemen while lowlands make slaves." It was found that this was only half of the truth, that it took account only of the objective environment, while an equally potent factor is the subjective environment: *cœlum non animum mutant qui trans mare currunt.* Character, however acquired, is difficult to change and must be reckoned with in any attempt to interpret human events. Thus expanded, the study of society from this point of view becomes a true science, and recently it has been given the appropriate name of *mesology*. The great influence of climate and physical conditions must be fully recognized. It reaches back into the domain of ethnology and physiology and doubtless explains the color of the skin, the character of the hair, and the general physical nature of the different races of men. The psychic effects of the environment are scarcely less important, and the qualities of courage, love of liberty, industry and thrift, ingenuity and intelligence, are all developed by contact with restraining influences adapted to stimulating them and not so severe as to check their growth.

The social effects are still more marked. We first see them in the phenomena of migration and settlement and the ways in which men adapt themselves to the conditions, resources and general character of the region they may chance to occupy. The question asked by the traditional boy in the geography class, why the large rivers all run past the great cities, illustrates how clearly everybody sees natural law at work in society. It is the laws of society that determine the direction in which population moves. For example, in peoples at all advanced the head of navigation of rivers is usually the site of the principal towns. A short time ago, when water was more used than now as a power, there was usually combined with the advantages offered by the head of navigation (vessels being then smaller than now), the additional advantage of the fall in the stream, which is almost always greatest at the point where the piedmont plateau joins the coastal plain. As streams only reach their base level after emerging upon the coastal plain, this sudden fall almost always occurs a short distance above the head of navigation. As this is true of all the streams that drain a continent, a line may be drawn through this point on all the rivers and it will be approximately parallel to the coast. Such a line is called the "fall line," and it is a law of population that the first settlements of any country take place along the fall line of its rivers.

There are many laws that can be similarly illustrated, and careful observation reveals the fact that all social phenomena are the results of its laws. The one example given must suffice in the present case. But these social, or sociological, laws may themselves be grouped and generalized, and higher laws discovered. If we carry the process far enough we arrive at last at the fundamental law of everything psychic, especially of everything affected by intelligence. This is the *law of parsimony*. It has, as we shall see, its applications in biology, and its homologue in cosmology, but it was first clearly grasped by the political economists, and by many it is regarded as only an economic law. Here it is usually called the law of *greatest gain for least effort*, and is the basis of scientific economics. But it is much broader than this, and not only plays an important rôle in psychology, but becomes, in that collective psychology which constitutes so nearly the whole of sociology, the scientific corner stone of that science also.

We have seen that the quality of scientific exactness in sociology can only be clearly perceived in some of its higher generalizations, where, neglecting the smaller unities which make its phenomena so exceedingly complex, and dealing only with the large composite unities that the minor ones combine to create, we are able to handle the subject, as it were, in bulk. Here we can plainly see the relations and can be sure of their absolute uniformity and reliability. When we reach the law of parsimony we seem to have attained the maximum stage of generalization, and here we have a law as exact as any in physics or astronomy. It is, for example, perfectly safe to assume that under any and all conceivable circumstances, a sentient and rational being will always seek the greatest gain, or the maximum resultant of gain—his 'marginal' advantage.

Those who affect to be shocked by such a proposition fail to understand it in its full breadth. They think that they themselves at least are exceptions to the law, and that they do not always seek their greatest gain, and they give illustrations of actions performed that result in a loss instead of a gain. This is because they understand by gain only pecuniary gain, or only gain in temporary enjoyment or immediate satisfaction. If they could analyze their feelings they would see that they were merely sacrificing a present to a future advantage, or what they regard as a lower to what they regard as a higher satisfaction. When Henry Clay said (if he did say it) that 'every man has his price,' he may have merely stated this law in a new form. If we make the important qualification that the 'price' is not necessarily a money price, we may see that the statement contains a truth. Even in the lobby, which he probably had in mind, it is well known that downright bribery is very rarely resorted to. It is among the least effective of the lobbyist's methods. There are other far more successful as well as less expensive ways of gaining a legislator's vote. Passes on railroads and other favors of that kind are much more common, but even these are relatively coarse and transparent, and the great vested interests of a country know how to accomplish their ends by much more subtle means. It is only necessary to put those whom they desire to influence under some form of obligation, and this is usually easy of accomplishment. Among the most effective means to this end are social amenities and the establishment in apparently the most disinterested ways of a friendly *entente*, which appeals to the sense of honor and makes any man ashamed to act contrary to the known wishes of a friend. Under such imperative influences as these constituencies are easily forgotten.

But this is by no means the whole meaning of the law. It deals solely with motives, and worthy motives are as effective as unworthy ones. It is based, it is true, on interest, but interest is not always bad. It is much more frequently good. It was necessarily good, at least for the individual, in the beginning, since it had the mission to impel life and race-preserving activities. Interest may be perverted, but this is the exception. Men feel an interest in doing good, and moral interest is as real as any other. Ratzenhofer shows that men have been profoundly moved by what he calls 'transcendental interests,' which he defines as a reaching out after the infinite, and to this he attributes the great religious movements of society. If therefore we take into account all these different kinds of interest, physical, racial, moral, social and transcendental, it becomes clear that all action is based on supposed gain of one or other of these orders.

Still, the world has never reached a stage where the physical and temporary interests have not been paramount, and it is these upon which the economists have established their science. Self-preservation has always been the first law of nature, and that which best insures this is the greatest gain. So unerring is this law that it is easy to create a class of paupers or mendicants by simply letting it be known that food or alms will be given to those who ask. All considerations of pride or self-respect will give way to the imperious law of the greatest gain for the least effort. All ideas of justice which would prompt the giving of an equivalent vanish before it, and men will take what is proffered without thought of a return or sense of gratitude. In this respect men are like animals. In fact, this is precisely the principle that underlies the domestication of animals and the taming of wild beasts. So soon as the creature learns that it will not be injured or molested and that its wants will be supplied, it submits to the will of man and becomes a — parasite. Parasitism, indeed, throughout the organic world is only an application of the law of parsimony. Pauperism produced in the manner described is social parasitism. But parasitism always results in degeneracy, and pauperism, engendered in society by well-meaning persons ignorant of the law of parsimony, is social parasitic degeneracy.

While, therefore, in view of the number and variety of causes that combine to determine any single act, no law can be laid down as to how any individual will act under a given set of circumstances, we have a law which determines with absolute certainty how all men act under all circumstances. If there is any apparent exception to this law we may be sure that some element has been overlooked in the calculation. Just as, in the case of a heavenly body which is observed to move in a manner at variance with the established laws of gravitation and planetary motion, the astronomer does not doubt the universality of those laws, but attributes the phenomena to some undiscovered body in space of the proper size and in the proper position to cause the perturbation, and proceeds to search for that body; so in human society, if there are events that seem at variance with the fundamental sociological law of parsimony, the sociologist may safely trust the law and proceed to discover the cause of the social perturbation.

July 31, 1902—Ætat. 61.

486. Review of Homeric Society. A Sociological Study of the Iliad and Odyssey. By Albert Galloway Keller, Ph.D. New York and London: Longmans, Green & Co., 1902

History.—Written April 11–12, 1902. I received this book on February 14, 1902, from Mr. F. W. Hodge, Editor of the *American Anthropologist*, who asked me to review it. I was so pleased with it at first sight, as based largely on a favorite theory of mine of the proper method of studying the sociology of antiquity, that I made it family reading, and Miss Sarah E. Simons joined my household on Sunday evenings and at half-hour daily sessions until March 2, to hear me read it and to discuss its various interesting points. The review was delayed by other work, but I finally reached it and soon dispatched it. I sent it to Mr. Hodge on April 14. The proof was read on June 7, and the April–June *Anthropologist* appeared on July 31.

The American Anthropologist, New Series, New York, Vol. IV, No. 2, April–June, 1902, pp. 292–295.

February 18, 1903—Ætat. 61.

487. Pure Sociology. A Treatise on the Origin and Spontaneous Development of Society

History.—Written July 14, 1901 to June 18, 1902. Preface written August 17 to 22, 1902. Table of contents prepared August 3, 1902. Index written January 1–19, 1903.

After the appearance of my *Outlines of Sociology* in 1898 (see supra, No. 452) and the delivery of my lectures on Sociology at Hartford in 1894 and 1895, at Chicago in 1897, at Morgantown in 1898, and at Stanford University in 1899, I began to plan the preparation of a systematic work on the general subject, and as I had always in my lectures divided the subject into two parts, in fact given two courses, one on pure sociology and one on applied sociology, I conceived that the new work should similarly be divided into two parts or consist of two volumes.

But in so large an undertaking I felt the need of advice and council. It was near the end of the year 1900, that my decision in the matter was finally taken, and as that year was the last of the nineteenth century, I thought it fitting to begin such a work on the first day of January, 1901, or the first day of the twentieth century. Although I had not yet written a word, I sent out invitations on December 27, 28, and 29, 1900, to eleven of my friends whom I knew to be interested in my previously published works, and with whom I was on more or less intimate terms in the discussion of high intellectual questions and the general exchange of views. The names of these persons are given in the preface to the second edition together with a general account of the circumstances, which therefore need not be repeated here. In inviting them I told them what the object of the gathering was, and that I needed their advice in so important an undertaking. All but three of the invited guests came. Dr. Harris sent regrets that his health did not permit him to go out evenings, Mr. Hutcheson was sick in bed, and Dr. Falkner was not in Washington at that time. On January 1, I drew up title-pages of both volumes, which I regarded as actually breaking ground in writing them, and as the commencement of the work, although, as we shall see, the serious work of writing the books was begun considerably later. The title of this work, as then drawn up, was simply: *A System of Pure Sociology.* Major Powell and some others questioned the propriety of using the word "System" in the title, and it was ultimately changed in this respect. In fact, it will be seen that two changes were made in the title before the final form was decided upon. I proposed to write the books very largely from my lecture notes, which had now accumulated till they assumed large proportions, and I brought out the boxes in which I kept them and showed them to the assembled guests. A fine feeling prevailed, and the affair gave me a new sense of confidence in my enterprise.

Three causes contributed to the delay in writing the book. In the first place my report on *Sociology at the Paris Exposition of 1900* (see supra, No. 481) was in hand and must be finished; secondly I had arranged to revisit the region of the petrified forests and make a thorough study of the geology of the Little Colorado Valley (see supra, No. 478, infra, No. 519), and after my experience in November, 1899, I decided to go in the spring, hoping that it might be less dry, an illusive hope; and thirdly, I wanted to extend my notes, adding largely to those I had used in my lectures, and to rearrange them in harmony with the new system that I had in mind. The first of these causes has already been sufficiently treated. The second simply meant an absence from home from May 3 to June 28. As regards the third something needs to be said at this place.

Early in my literary career it became evident that I could not trust my memory in finding passages in books that I read, and I adopted a system of recording them in such a manner that I could refer to them. It was of course the commonplace book method, and I reflected some time on the kind of book best adapted to that purpose. One day (it was June 27, 1870) I saw in a bookstore a book with the following title: *Index Rerum; or, Index of Subjects; intended as a Manual to aid the Student and Professional Man in preparing himself for Usefulness. With an Introduction, illustrating its Utility and Method of Use.* By John Todd, D.D. Northampton, Mass.: Bridgman and Childs, 1868. I will not enter into a description of it, further than to say that it is a blank book with the letters of the alphabet in the upper corners of the pages, these letters always consisting of a vowel in lower case prefixed to the capital letter of the alphabet so as to give about two pages to each such combination, and it is explained that the vowel refers to the first vowel after the first letter of any word. I bought a copy of this book and proceeded to use it in my work. I then imagined that it would last me my lifetime, for I did not follow Todd's directions and make quotations from authors, but confined myself exclusively to giving abbreviated references. In a few years it was practically full, so that I had no room for new entries, and I got myself a large ledger with 600 pages and laid it off carefully, abandoning Todd's method, and proceeded to use it. But even that has now long been full, and the third book was opened on January 1, 1907. Throughout all this period every book I have read has been posted into these commonplace books, so that everything that I considered worth preserving, or at all likely to be wanted, may readily be found. But naturally many very similar subjects would be entered under different heads, and down to the time of which we are now treating no serious effort had been made to correlate and coördinate the matter of these books. I now saw the need of this, and I devoted a long time to this work during the winter and early spring of 1901. I did some work on it nearly every evening from January 23, to April 28. Many new slip notes were also written, and these were combined with all my lecture notes. This work included the classification of all this slip matter in the order in which I expected to arrange the subjects of both the *Pure* and the *Applied Sociology*. To this end I had made a preliminary list of the chapters, or provisional table of contents, numbering the chapters of each volume, and then I went through the slips and assigned each one to its appropriate volume and chapter, and arranged them in this order. All this was done before I left for the field.

Although I was deeply interested in my field work on that campaign, still my mind constantly ran on my books, and after I lay down for the night on the ground, in the open air or in the tent, I would think myself to sleep by dwelling on the various topics that I wanted to discuss, and by trying to perfect their classification and arrangement. At last I came to make it a regular duty every night to go through the whole contents of the volumes, and after I had decided on the form of title for each chapter, to repeat the titles to myself until they became perfectly familiar. I did not write a word of it, but simply memorized it, until the exact phraseology of every chapter and section head was fixed in my mind. Changes of course I would often make, but always, as I supposed, for the better. I finally had it all so perfectly settled that the preparation of a new table of contents, which was the first thing I did after my return, viz., on July 3, was a purely mechanical operation. The changes in the contents thus made involved considerable rearrangement of the slip notes, and it was July 14, before I was entirely ready to commence writing *Pure Sociology*. I will simply give the dates of the execution of each chapter. Chapter I was written July 14–16. What I intended to form Chapter II was written July 16, to August 27, but, as we have already seen (supra, No. 480), most of this was rejected and published in three magazine articles under the title: *Contemporary Sociology*. Chapter III was written September 5–15; Chapter IV, September 16–27; Chapter V, September 27, to October 19; Chapter VI, October 19–28; Chapter VII, October 29, to November 10; Chapter VIII, November 20–23; Chapter IX, November 23, to December 8; Chapter X, December 8–29. At the last-named date I halted to prepare a full table of contents to the end of Chapter X, giving heads and sub-heads and pages of the manuscript, with a column showing the length of each chapter. This was for my own use. Chapter XI was written January 1–19, 1902; Chapter XII, January 20–26; Chapter XIII, January 27, to February 7; Chapter XIV, February 8, to April 8; Chapter XV, April 13–30; Chapter XVI, May 1–7; Chapter XVII, May 7–13; Chapter XVIII, May 13–18; Chapter XIX, May 18, to June 7; Chapter XX, June 7–18. Thus the actual execution of the work fell a little short of eleven months. June 18, 1902, was my sixty-first birthday.

At that date I had decided to change the title to: *Pure*

Sociology: A Treatise on the Origin and Nature of Society. I had covered so many subjects in the book, and as I did not know how long it might take to find a publisher, I concluded to index the entire manuscript on slips that could be used for printed pages whenever these were ready, and I commenced this prolonged task the next day after the last chapter was finished. It took me more than a month (June 19, to July 24). In writing the book I had made many references and quotations at second hand without stopping to find the originals in libraries. I went through the manuscript and took out all folios containing such unverified matter and all on which there were incomplete references, and commenced a systematic search for them wherever they were to be found. This was a prolonged labor and occupied me from July 28, to August 19. On August 17, I wrote the preface, and on that day I also wrote a letter to the Macmillan Company offering them the book. They replied on August 21, asking me to send them the manuscript. I signed the preface on August 22, and sent the manuscript on August 23. By a curious accident, or blunder on my part, I failed to include in the package sent the envelope containing the preliminary pages (preface, contents, etc.). I did not discover the mistake till August 26, and I sent it on that day with all due apologies. But meantime they had sent the manuscript to their referee, who proved to be none other than Dr. Small, as he afterwards told me himself. On September 8, I received a letter from Mr. Brett accepting the book. Articles of agreement were signed on the 11th and specimen pages sent me on the 17th. Proofs began to come on October 10. There were a few references that could not be verified in any of the libraries of Washington, and having business in New York and Boston I took them along and settled them, mostly in the latter city on October 7, but I was obliged to go to Norwood where the manuscript was, to introduce the corrections on the proper pages.

As fast as pages arrived I entered them in red ink on the slips of my index of the manuscript, and kept this work up, so that when the page proof was all in my hands and thus indexed on the slips I was ready to commence preparing the manuscript of the index. The work occupied from November 11, 1902, till January 19, 1903, but the actual writing of the manuscript of the index was begun on January 1, 1903. Proof of the index was corrected

January 25–28, certain plate corrections made February 3–10, and the book was published on February 18, 1903.

It cannot be said that *Pure Sociology* met with a very warm reception. It was too deep not only for the general public but also for the principal sociologists, very few of whom, and least of all the organicists, were biologists in any professional sense, and still less physicists, chemists, and astronomers. They really had little idea of scientific philosophy, and as Gumplowicz said of the book, it is more than sociology, it is cosmology.

I have already stated that Dr. Small was the publishers' referee, and they sent me some criticisms that accompanied his report, which seems to have been otherwise favorable. They did not say who he was, and I confess that when I read them; though I thought of Dr. Small, I did not recognize them as his. It may be appropriate, as partly explaining his subsequent attitude toward the book, to introduce them here. In a letter dated September 10, 1902, Mr. George P. Brett, President of the Macmillan Company, who signed the letter, said:

"Our reader's suggestions are given below and we shall be glad if you will give them due consideration as they come from a man who without doubt is well qualified to speak on such matters. It must be understood, however, that we do not endorse in any sense this opinion and only submit it to you for such use as you may be pleased to make of it:

"'There seems to me to be a slip on p. 251 [194 of the printed book] in putting a little too much rhetoric into the stricture on the logic of educational programs. The statement is too severe and sweeping. The essential justice of the criticism will lose its point if the overstatement stands.

"'I would suggest that in connection with the discussion of sympodial branching—pp. 311 et passim—[71 ff] he might appropriately interpolate a remark which would relieve the minds of the people who are not evolutionists because they can't be reconciled to the idea that their ancestors were monkeys.

"'Also in his discussion of Comte's dream of parthenogenesis it might not be out of place to refer to Professor Loeb's development of the unfertilized eggs of sea-urchins as due to possible future discoveries of a more important character in the same direction.

"'Of more importance than these details is the following: I assume that there will be a full index of course, and I hope that there will be also a synoptic table of contents or conspectus of the material. Besides that I earnestly recommend that a chapter or chapters be added on the bearing of *Pure Sociology* upon ethics.

"'I do not agree with the author in excluding ethics from pure sociology. Indeed I do not think he is quite consistent with himself in that exclusion (viz. p. 303 [69] where he makes ethics "only a department of sociology"). If he wants to make the line severely exact between "pure" and "applied" sociology, and wants to include ethics under the latter head, my quarrel with him is not serious. He might compromise by putting what I want, if he preferred, in the form of an appendix. The fact is that he makes on almost everybody the impression that his sociology is essentially archæologic and static, whereas in his own mind it is essentially dynamic. In his zeal to guard the thoroughgoing scientific character of his system he virtually leans over backward and doesn't let people see his real objective. It would add greatly to the value of this book if he would show what it all amounts to as prolegomena to a methodology of positive ethics.

"'He could say what he thinks on the subject of The Significance of Sociology for Ethics in outline in much less space than 20,000 words, because the chapters already written contain what it is necessary to indicate. He could do all that by references.

"'I hope he will give this suggestion due consideration for I am sure the adoption of it would not only add greatly to the practical value of the book for almost all readers, and [but] it would bring out his own ideas of emphasis and proportion and correlation.'"

This same letter enclosed the articles of agreement for the publication of the book, so of course none of these extensive alterations and additions were attempted. It would be easy to show on every count that they would be no improvement. As for the traditional ethics, I have never made my bow to it, and have always maintained that sociology is a science of liberation and not of restraint.

I know not how it happened, but there was something in *Pure Sociology* that alarmed the "vested interests" of the land. The word seems to have been passed along among the organs of the "subsidized press" to give it a "cold shoulder." The first indication of this was the humorous review in the New York *Sun* for March 1, 1903, the writer of which had at least read the *index*. But he quotes two somewhat extended passages supposed to show that the book was a mass of bombast and "fish stories." The one taken from p. 465 is the clearest distinction I have ever drawn between genetic and telic phenomena, and is one of the few eloquent passages in the book. As to the one on p. 182, it describes an event in my own life which is as vivid in my memory to-day as it was immediately after it occurred. The facts are not at all exaggerated. I did not take space to show that many animals continue to grow as long as they live. Spencer (Biology, I, 126) mentions the fact and cites the crocodile and the pike. Semper, Whitfield, Varigny, and Cuenot all show that in cultivating fishes their size is largely determined by that of the aquarium. It was, in fact, an important scientific observation made by a boy of 13.

This burlesque review of the *Sun* was ably replied to in the Boston *Transcript* for April 9, 1903, by Laura McAdoo-Triggs. A great many other reviews appeared which cannot be even mentioned here, but a considerable number were confined to a discussion of Chapter XIV. In speaking of these latter I might mention the rhyme that appeared in the London *Punch* (Vol. CXXV, August 5, 1903, p. 79), and is as follows:

GYNÆCOCENTRICITY

[Mr. Lester F. Ward in his "Pure Sociology" advocates the "gynæcocentric theory," in which he maintains that woman is primary and essential; that originally and normally all things centre about her, and that man is a mere after-thought of Nature.]

Hence, androcentric theory,
  Of ignorance and male perverseness born,
  That doomst me night and morn
To endless labors, masculine and dreary.
  Cribbed in some city den,
Where fog and darkness spread their sooty wings,
And the typewriter rings,
Thou bidst me toil and slave the long day long
Amid the madding throng,
  With painful care driving a clerkly pen.

But come thou system, called by me
Sweet Gynæcocentricity!
Make me as a cypher, nought
But a trifling after-thought,
While to woman you restore
All the might was hers of yore.
Once again command that she
Man's support and centre be,
Guiding with her wiser powers
All her own affairs and ours.
I would cling to MARY ANN,
I the woman, she the man;
Independence I would drop,
She the pole and I the hop.
Every privilege my sex
Would from MARY ANN'S annex
I would yield her up and be
Trampled under foot as she.
I would see her, sun or rain,

Hurry for the early train,
And only leave her desk to crunch
At 2 p. m. her lightning lunch.
Meantime I with prudent care,
To my work-box would repair,
Draw my knitting from the box,
Or proceed to darn the socks,
Or the garden I would seek,
Where soft Zephyrs fan the cheek;
There within the checkered shade
Which the weeping willows made
In my swinging hammock I
With my favorite books would lie,
And read and meditate and moon
Through all the lazy afternoon.

This give and I will live with thee,
Sweet Gynæcocentricity!

It is a sort of parody on Milton's *Allegro*, and is cleverly done. It was widely copied by the press.

Dr. Small published a short announcement of the book in the March number of the *American Journal of Sociology* (Vol. VIII, p. 710), in which he said that a group of writers would be invited to discuss it in that *Journal*. If they were invited they never came forward, and the only treatment it there received was a review in the number for November, 1903 (Vol. IX, pp. 408–415) by H. Heath Bawden of Vassar College, which shows that the writer had not the slightest conception of what the book was about, and three notes by Dr. Small in the numbers for November, 1903 (pp. 404–407), January, 1904 (pp. 567–575), and March, 1904 (pp. 703–707). These notes deal with certain special principles and are partly expository and partly critical. His real review of the book appeared in the *Political Science Quarterly* for June, 1904, Vol. XIX, pp. 318–319. It does not go to the merits, but is very complimentary. His use of my phrase "illusion of the near" with regard to it is amusing and may prove prophetic. He shares the current view that sociology is something much narrower, and classes me as a philosopher rather than a sociologist.

Among the letters received acknowledging the receipt of the book are two from Dr. Patten which were written after he had read it. They are very appreciative, and in the second one he refers especially to the doctrine of sympodial development as of great importance in its application to human affairs. Letters from Dr. De Greef and from M. Jacques Novicow express great interest in the treatment of the reproductive forces (Chapter XIV).

On December 24, 1906, I received a letter from the Macmillan Company informing me that they were about to publish a second edition of *Pure Sociology*, and on the 28th they asked me to send them a preface to the second edition. This was written on December 30, and the title-page drawn up, and both were sent on that day. On January 31, 1907, I discovered that in saying "seven" years at the beginning of the preface I had made a mistake of a year, as it was only six years. I wrote to know whether it was too late to correct it, and was told that it was. I imagine no one has ever noticed it but myself. The edition appeared on February 15. A large number of plate corrections of typographical errors were made. Several have been detected since.

In a letter to Dr. René Worms dated September 10, 1902, relative to the place of meeting of the next Congress of the International Institute of Sociology, I incidentally mentioned the matter of a possible French translation of *Pure Sociology*. On October 3, I received one from him in which he asked me if I could translate it. I replied that I would not like to undertake so large a task, but that I would revise a translation made by another. I obtained from the Macmillan Company a waiver of all claims and full permission to have the work translated. Dr. Worms arranged with Messrs. Giard et Brière for the publication of the translation if a translator could be found. The matter had reached this stage before I arrived in Paris on June 17, 1903. Nothing could be done until after the adjournment of the Congress over which I had to preside, which was on July 9, but on the 11th I went with Dr. Worms to see M. Brière, now head of the firm, and the matter was definitively settled. Dr. Worms had found a young man, M. Fernand Weil, who understood English, and who arranged with the publishers to translate the book. On July 13, he called on me in my room and I gave him a copy of the book to translate. He commenced work on it on July 15, and on the 20th brought me his translation of the first two chapters. There were certain conditions imposed by the publishers, one of which was that I should reduce the size of the book by omitting a certain amount of the matter, and on July 29, and 30, I went through M. Weil's copy and marked the parts that could best be spared. This I greatly disliked to do, and it renders the French edition much inferior to the English. I remained in Paris till September 23, and M. Weil brought me instalments of his translation to revise. He had then finished the first thirteen chapters, which is the point at which the first volume of the French edition ends. He gave me the whole of this to take to America and subject to a second and more thorough revision. M. Weil is a scholarly young man, but has had no scientific training, and the great difficulty was with the scientific terms. I generally knew them better than he.

After my return, on October 23, I commenced this final revision. It proved an immense work, but by devoting an hour or more to it nearly every evening I succeeded in completing this first half on January 27, 1904. I had asked M. Weil to leave blank space for all the passages that I had translated from French works for me to supply the original, but he had translated many of them, and for this the original had to be substituted. I did not have all these books in my house, and was obliged to hunt them up in libraries. On February 3, 1904, this work was completed, and I sent back the manuscript through the International Exchange Bureau of the Smithsonian Institution. On February 25, I received the second half of the French translation from M. Weil. This had to be treated in the same way, but I gave more time to it, and finished and returned it on April 29. Progress in the composition was reported to me from time to time by both M. Weil and Dr. Worms. Volume I was in type by the end of 1904, and, in conformity with a rule of the Bibliothèque Sociologique Internationale, in which this work is included, and of which this volume was the thirty-second, Chapter III was published as the leading article of the *Revue Internationale de Sociologie* for January, 1905. The work appeared in November, 1905, and a portion of Chapter XIV, which is the first chapter of the second volume, was made the leading article of the *Revue* for that month.

The title-page of the French translation of *Pure Sociology* contains the following words: Bibliothèque Sociologique Internationale | XXXII (in the first volume) XXXIII (in the second volume). Sociologie Pure | par | Lester F. Ward | membre et ancien président | de l'Institut International de Sociologie |—Traduit de l'anglais, avec le concours de l'auteur, | par | Fernand Weil | membre de la Société de Sociologie de Paris |—Tome Premier—Tome Second—Paris (5^e) V. Giard & E. Brière | Libraires-Éditeurs | 16, Rue Soufflot et 12, Rue Toullier |—1906 | Tome Premier, pp. iii+361; Tome Second, pp. 381. 8°. In brochure and also bound. I received it in both forms, but had my brochure volumes bound. I received the unbound volumes Dec. 18, 1905, and ordered the bound ones, which arrived on Feb. 1, 1906.

On September 13, 1903, when I was in Paris, I received a letter from Mrs. Johanna Odenwald Unger, addressed to me in Washington from Sonnewall, Duisburg am Rhein, August 22, 1903, and forwarded back to me in Europe, in which she inquired whether my works had been translated into German, virtually offering to translate them. This led to further correspondence on that side, and Mrs. Unger arranged to return to America on the same steamer with me in October. On the voyage I explained to her that a French translation of *Pure Sociology* was being made, and showed her the manuscript that I was taking back with me for revision. She offered to undertake to translate it into German if a publisher could be found. Professor Gumplowicz had, I think, said something to me about asking his son to translate it, and on October 15, I wrote to inquire about it, and told him that a German-American lady was willing to undertake it. On November 9, I received a letter from him saying that he had said nothing more to his son about it, and that I could let the lady go ahead. In fact, as I see from her letters, she began the translation on October 19, 1903, and she reported Part I finished on November 25. On February 19, 1904, she commenced sending me her manuscript for revision. She had then translated ten chapters. Her husband, Mr. John S. Unger, who is an engineer, was kind enough to draw the dial on page 40. I proceeded to revise the translation, much as I had the French, but Miss Sarah E. Simons came evenings and assisted me. On May 6, 1904, Mrs. Unger reported the completion of Chapter XIII, which, as in the French translation, forms the last chapter of the first volume. I received Chapter XV for revision on September 14. As I was going to the World's Fair in St. Louis, we arranged to meet there and compare notes. This we did on September 22, and she had with her the manuscript as far as translated. We settled a good many knotty questions then. After my return I went on revising and finished Part II on October 8, and Part III on December 1. In a letter dated November 24, 1904, she says:

> "At last the great work is finished and all your beautiful thoughts—which are nothing but the truth all through—are ready to go to the world again in a new garb."

I sent each instalment back to her as soon as revised, and she put on the finishing touches. On February 20, 1905, she reported all completed and ready for the press.

On November 25, 1904, I received a letter from Professor Gumplowicz in which he offered to do everything in his power to have the German translation published, mentioning a number of publishers, including his own, the Wagner'sche Universitäts-Buchhandlung in Innsbruck. He suggested that I first offer it to Brockhaus in Leipzig, and I wrote to Brockhaus on March 8. On April 3, I received a letter from Brockhaus declining to publish the translation on two grounds; first, that he had too much matter already in hand, and second, that he had always made it a rule not to publish translations except simultaneously with the original. I therefore wrote at once to Gumplowicz to go ahead with the other publishers. He laid the matter before his publishers at Innsbruck and they consented quite willingly to undertake it. I corresponded with Mr. Eckart Schumacher, the active head of the firm, on the subject and sent him Mrs. Unger's address. There was some correspondence between them relative to an honorarium, and finally, on July 10, 1905, she sent on the first half of the manuscript. On October 12, the proof began to come. They sent it to her and she corrected it and sent it to me. I carefully read it and returned it direct to the publishers. The composition was very slow and the first half of the book was not yet in type when I went to Innsbruck and called on the publishers on June 22, 1906. They treated me very cordially, and the elder Schumacher, father of Eckart, was especially kind to me and showed me all about the city. All seemed to be going well to that date.

The matter dragged on, proofs coming at long intervals till January, 1907, when Eckart Schumacher wrote to Mrs. Unger that they could not send proofs to America any more, and suggested that Gumplowicz be asked to correct the proofs. On January 25, 1907, I wrote to Gumplowicz, enclosing letters from Schumacher, and asking him if it would be too great a burden to correct the remainder of the proofs. He replied, expressing entire willingness to do so. After February of that year they were sent to him. They sometimes sent him duplicates, and these he would send to me. Of course any corrections I might make in these could only be made in the electroplates. I do not think they were made, although I found many errors. On June 15, 1907, I received from Mrs. Unger translations of the preface and title-page, which I sent to Gumplowicz the next day and asked him to examine them and forward them to the publishers.

It was understood that the German edition was to consist of two volumes, the first to end with Chapter XIII, as in the French edition, but I supposed they would follow the same plan as the Paris publishers, and let both volumes come out together. But on July 4, 1907, I received a letter from Schumacher saying that he had sent me Vol. I, and would resume work on Vol. II at once. The volume, unbound, reached me the next day. On the title-page there is an inexcusable mistake in Mrs. Unger's first name, "J. V." for "J. O.," but it should have been Johanna Odenwald. There was another long delay in the proofs, none reaching Gumplowicz till September, and then they soon ceased again. More than a year later Schumacher apologized to Mrs. Unger for having mislaid a portion of her table of contents, causing several months' delay. At last, at the end of that year (1908), the second volume was announced, and on February 18, 1909, a copy of it reach me.

The two volumes taken together constitute the most unsatisfactory piece of bookmaking with which I am acquainted. Desperate efforts were made to have the blunders in the title-page and elsewhere of the first volume corrected in the second. No attention was paid to our representations. Although the two parts were called volumes and special title-pages were placed in each, still the pagination was made continuous through both volumes, the second beginning with page 363. I went carefully through both volumes and prepared a long list of errata. Every page bristles with typographical errors. I sent the list to Mrs. Unger and she sent it on and it was printed. They were asked to put a copy of it into all the copies in hand. On April 1, Schumacher wrote to Mrs. Unger saying that the book was a financial failure and asked her to waive her honorarium! The one fact that the first volume was published nearly two years in advance of the second, or that it was published separately at all, was sufficient to make the work a financial failure. No one would buy one volume of what was to be a two-volume book, and by the time the second volume appeared all interest would have ceased. The whole operation may be described as a "botch." And yet, as the book contains the whole of *Pure Sociology*, while the French edition is badly cut down and crippled, I prefer the German to the French edition.

. The work bears the following title: Reine Soziologie. | Eine Abhandlung | über den | Ursprung und die Spontane | Entwicklung der Gesellschaft. | Von | Lester F. Ward. | Aus dem Englischen übersetzt von J. V. [O.] Unger. | (vignette) Innsbruck | Verlag der Wagner'schen Universitäts-Buchhandlung. | [I. Band] 1907. [II. Band] 1909. |

On March 20, 1905, I received a letter from Prof. Adolfo Posada, dated March 2, saying that a publisher who was a friend of his desired to publish a Spanish translation of my *Pure Sociology*, and asked my authorization to do so, which, of course, I freely gave. I have heard nothing further of it except that in a much later letter (June 9, 1906) Professor Posada says: "I think, like you, that it will be better to translate the *Pure Sociology* before the Text-book." This shows that at that time the translation of my works into Spanish was going on under Professor Posada's direction.

On February 17, 1906, I had a call from a Japanese named Unoské Wakamiya, who had read my books and expressed a desire and even the intention to translate my *Pure Sociology* into Japanese. We conversed some time and other writings of mine were mentioned. At any rate I gave him a copy each of my *Contemporary Sociology* (see supra, No. 480) and *Herbert Spencer's Sociology* (see infra, No. 498). I heard nothing more from this until, in a letter dated January 10, 1908, Mr. K. Kaneko, then in Chicago, said:

> "Perhaps you know my old friend Unoské Wakamiya who is now in Japan and is preparing to introduce your works to the Japanese public. A few days ago I received a letter from him in which he says that the first part of *Pure Sociology* will come out by some time in January, with his 50-page long introduction as well as a chapter on *Contemporary Sociology*. He said he was obliged to divide the book into four parts in order to get them out easily and cheaply. He is doing this at his own expense. I am glad to know that your valuable work is going to be introduced to the Japanese reading public through the efforts of a friend of mine."

Mr. Wakamiya had translated a book by George Knox on *The Spirit of the Orient*, and in the Japanese edition he inserted an announcement of his translation of *Pure Sociology*. Mr. Kaneko sent me a copy of this announcement in Japanese. Not being able to read it, I sent it to Commander Naomi Taniguchi of the Imperial Japanese Navy in Washington, whom I knew, and he sent me a translation of it. I need only say that it is highly complimentary to me. Mr. Kaneko sent me Wakamiya's address and said he hoped I would write to him and encourage him. On March 29, I wrote to Wakamiya, asking him to send me his translations of all my works and telling him that I would see that he was remunerated. On July 6, I received a letter from Kaneko, saying that Wakamiya was sick and in a hospital, and had not yet finished the translation of *Pure Sociology*. Nearly a year more elapsed before I heard anything further from the matter, but on June 2, 1909, I received a letter from Wakamiya dated Gugakuji, Kamakura, Japan, May 7, 1909. He had received my letter, but had been very ill in a hospital and unable to do any work. He hoped, however, soon to resume his literary work. He said that, besides the two articles, one third of *Pure Sociology* was translated. He goes on to say that there appears to be no market for such a work in Japan, and small chance of finding a publisher. He was thinking of starting a magazine and hoped to be able to let the work run through numbers of that.

April, 1903—Ætat. 61.

488. Administrative Report for the Year Ending June 30, 1902

History.—Written June 14, 1902.

Twenty-third Annual Report of the United States Geological Survey, 1901–02, p. 70.

May 22, 1903—Ætat. 61.

489. Social Differentiation and Social Integration

History.—Written September 12, 1902, to March 26, 1903. This article was written wholly for the purpose of presenting it to the Congress of the International Institute of Sociology (see infra, No. 490). I followed my customary rule in writing French papers and first prepared an English draft. I finished this on September 19, except the quotations that I wished to make from Mr. Holmes's address, and proceeded to write the French draft. The footnote at the end of the English article was not included in the French paper. On October 1, I received a copy of Mr. Holmes's address and on October 3, I finished the English draft except the above-mentioned footnote. On October 20, I handed in a title of a paper that I offered to read before the Anthropological Society of Washington, viz., *Race Differentiation and Race Integration*, and on November 4, I read this paper using parts of the article. But I had had an enlarged dial prepared, which I hung up and discussed, and put the numerical data that appear on p. 739 on the blackboard. I treated the subject thoroughly, occupying about an hour. It happened that Prof. Edmund J. James, then president of Northwestern University, was present, and he was invited to speak and did so.

As this was virtually a translation of the French paper, its publication was not allowed by Article 9 of the constitution of the Institut until it had appeared in the *Annales*, and then only by stating that it was an *Extrait des Annales de l'Institut International de Sociologie.* I had never allowed the publication of my articles in English until I knew the exact volume and pages of the *Annales* in which they appeared, but in previous cases this brought the English article out somewhat later than the French. On March 16, 1903, I received a page proof of the French article, which enabled me to thus give the exact source. I then wrote to Dr. Small on the 19th, relating the circumstances, and asked him if he wished to publish the English translation. He replied that he would like to have it at once for the May number of the *Journal*. It was then that I appended the footnote, and I mailed the manuscript to him on March 27. It was put in type speedily and I received proof on April 18. The May number reached me on May 23, and my reprints on the 27th. This was about two months before the ninth volume of the *Annales*, containing the French article, appeared, and although that nominally precedes the English, I will follow the uniform rule applied to this work of arranging the titles in the exact chronological order of their actual appearance, and let the French article follow the English one.

The American Journal of Sociology, Chicago, Vol. VIII, No. 6, May, 1903, pp. 721–745; also covered reprint from the same.

July 4, 1903—Ætat. 62.

490. La Différenciation et l'Intégration Sociales—Une Utopie Sociologique

History.—Written September 19, to November 20, 1902. On September 10, 1902, I received a letter from Dr. René Worms, General Secretary of the International Institute of Sociology, saying that it had been the uniform custom of the Institute to have the leading article of its *Annales* in each year written by the president of the Institute for that year, and asking me, as I had been chosen president for the year 1903 (see supra, No. 476; infra, Nos. 502, 505), if I could contribute such an article. I had become deeply interested in this subject in writing Chapter X of my *Pure Sociology*, written nearly a year before, but especially by the facts treated in the postscript to that chapter, written much later, and still fresh in my mind, and I saw that I could combine this subject with the one treated in the footnote on pages 38–40 of that work, using the dial of my own devising, and make a very readable article. I decided upon the title and blocked out an English draft very quickly (see supra, No. 489). I then attacked the French draft and pushed it forward rapidly. The original draft written on alternate lines was finished on October 4, and submitted to Professor Maillet for correction and idiomatic improvement. There were delays, but I got it back on the 19th and commenced reducing it to final form. It was mailed to Dr. Worms on November 3. I had made a rough drawing of the dial with the lettering translated into French, which I enclosed and explained to Dr. Worms that I would send him one later fit for photographic reproduction. On the third I gave the data to my official draftsman and had a perfect dial drawn. It was done on the 10th and I sent it on the 11th. My part was then done, and I waited till March 16, 1903, when the proof arrived.

I reached Paris on June 17, but was busy with matters relating to the Congress, and did not go to the house of Giard et Brière till July 4. The *Annales* for 1903 (Tome IX) was then out and I obtained a copy. It may have appeared a few days earlier. My reprints arrived on June 7, in time for distribution at the Congress, which was done quite liberally, and many members complimented me on the appearance of the reprints, which certainly is unique, with the dial on the cover and title-page. Some members dropped me polite notes on their visiting cards. That of M. Charles Gide reads: "avec tous les remerciments pour son utopie scientifique qu'il a lue avec le même intérêt qu'un poème épique." I also sent them out to many persons at a distance, and received numerous responses. Ratzenhofer wrote me no less than three long letters about it, in a handwriting, which, though fine, is as perfect as copper plate, using the German script. He of course criticized my "utopia," because, as all know, he was strongly opposed to the mixture of races, and had false notions about its deteriorating effect, but he was much impressed with my views. Professor Posada contributed a 16-page article to *La España Moderna* of Madrid for December, 1903, on the *Utopia Sociológica*, dealing chiefly with my article.

Annales de l'Institut International de Sociologie. Tome IX, Travaux de l'Année 1902, Paris, 1903, pp. 49–85; Reprint (Extrait) with cover and title-page, entitled: La Différenciation et l'Intégration Sociales, par Lester F. Ward, Président de l'Institut International de Sociologie Cadran du Jour cosmique. Âge de la Terre (with diagram). Paris, 1903, pp. 1–39. 8°.

December 4, 1903—Ætat. 62.

491. [Ehrenberg's Scientific Prediction]

History.—This was a note presented to the Biological Society on November 14, 1903, and written out for the Secretary, November 18. I was interested in the nomenclature question and noticed this curious case during my study of the literature of diatoms.

Science, New Series, New York, Vol. XVIII, No. 466, December 4, 1903, pp. 722–723.

LESTER F. WARD noted a curious case of scientific prediction in which ten species were named and described before they were discovered. This was done by Ehrenberg, who in working on the diatoms, classed by him as infusoria, described a number of species of *Actinocyclus* based on the number of rays. For most of these he had specimens, but for the species with 27, 29, 30, 31, 37, 39, 41, 42, 44, 45, 46, 48 and 49 rays no specimens had at the time been discovered. Ten of these were found in 1843 and 1844, but the last four seem never to have come to light.

December 4, 1903—Ætat. 62.

492. The Dresden Cycad

History.—This is the note written out November 18, 1903, for the Secretary on my paper read before the Biological Society, November 14. The paper was soon published in full under the title: *A Famous Fossil Cycad* (see infra, No. 507).

Ibid., p. 723.

LESTER F. WARD presented a paper on "The Dresden Cycad" (*Cycadeoidea Reichenbachiana*), giving a brief historical account of the cycad trunk from the salt region of Galicia that has been in the Dresden Museum since its discovery in 1753. Its true character as a cycad was made known by Göppert in 1844, when he named it *Raumeria Reichenbachiana*, and he described and figured the specimen in 1853. More recently it had been photographed by Geinitz and a copy of the photograph sent to the speaker, who surmised that the cycad had been mounted in an inverted position, and so stated in his "Flora of the Black Hills." In August, 1903, Mr. Ward visited Dresden and had an opportunity to examine the specimen carefully, finding that it was really inverted. The speaker gave as minute a description of the trunk as was possible without cutting sections to show the internal structure, stating that it shows a number of reproductive organs that promise good results when they shall be cut through and examined microscopically.

493. [Herbert Spencer, his Death, his Visit to Washington, Some of his Last Letters, Sketch of his Career]

History.—On December 9, 1903, a reporter of the *Sunday Washington Post* named Hallock called and interviewed me relative to Herbert Spencer, whose death had recently occurred. He wanted to see letters from him and other things that I had to show him, but which I wanted a little time to get together, and he called a second time. I then showed him his last letter to me, which he copied and it appears here. He also copied my letter congratulating him on the completion of his *Synthetic Philosophy* (see supra, Vol. V, No. 435), and several other things, including the menu at the Herbert Spencer Banquet in New York (see supra, Vol. III, No. 137), which did not get printed. He took down in shorthand from dictation the parts in quotation marks, and they are reported and printed as nearly as I dictated them as could be expected. I have struck out Darwin's name in connection with the law of evolution, which I am sure I did not use, but the popular notion that Darwin discovered that law is so widespread that it is difficult to prevent newspapers from repeating it.*

The Washington Post, No. 10046, Sunday, December 13, 1903, Second part, p. 10.

ALTHOUGH the fact is to-day well-nigh forgotten, and little or nothing was known of it at the time, Herbert Spencer, the greatest philosopher of the nineteenth century, who died at his home in Brighton, England, on last Tuesday morning, visited and spent three days in Washington in the early part of September, 1882. He was a personal friend of the late Dr. G. Brown Goode, of the Smithsonian Institution, and for the last twenty-one years of his life he remained in correspondence with Prof. Lester F. Ward, curator of paleobotany of the National Museum, one of the foremost American scholars and authorities on social philosophy, and the author of "Dynamic Sociology," "The Psychic Factors of Civilization," "Outlines of Sociology," and "Pure Sociology." Prof. Ward received his first letter from Herbert Spencer in 1883, when his work, "Dynamic Sociology," appeared, while the final letter which he received from the great philosopher and which was undoubtedly the last he ever wrote to any person on this side of the Atlantic, arrived as late as March, 1903. In fact, investigation may show this to have been the last letter ever written by Herbert Spencer to any one. The epistle in question is one in which Spencer acknowledges receipt of Prof. Ward's last book, and runs as follows:

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* Mr. Grant Allen has emphasized this in his article on "Spencer and Darwin," *Fortnightly Review*, February, 1897, p. 253. *Popular Science Monthly*, April, 1897, p. 817.

PERCIVAL TERRACE, BRIGHTON,
March 16, 1903.

MY DEAR SIR:

This morning there reached me a copy of your large and elaborate work on Pure Sociology. Had it come ten years ago I might have been able to bestow upon it the attention it deserves, but now that I am eighty-three within a month, my energies alike for reading and thinking have fallen to a very low ebb. Dipping here and there on points especially interesting to me is all I can promise myself, and, unfortunately now, any resulting instruction comes too late to be of any service to me. Sociology with me has now come to be confined to comments upon the incidents reported in the daily journals.

I perceive, however, that your conception is large and philosophical, and cannot be otherwise than beneficial as a factor in the progress of thought.

Sincerely yours,
HERBERT SPENCER.

VOYAGE FOR HEALTH

Speaking of his correspondence with the great English savant, of his first meeting with him, and of his impressions of the character of the man and his influence on thought and civilization, Prof. Ward said:

"Spencer was in failing health for a great many years before his death. Although a man of moderately good constitution, it seemed as though the enormous work and thought involved in the production of his 'Synthetic Philosophy' was more than mortal could stand, and since the completion of the work in 1897, he showed steady mental decline. Formerly, when a young man, he did his own writing, but for some years prior to his death, his mind gave way so badly that he could only dictate ten minutes at a time, with an hour's rest between times. Moreover, he could work in this way only in the mornings, for by the time the afternoon was arrived, he was tired out and had to rest. In short, since 1880 he had been in declining health, and his trip to the United States in 1882 was in the hope of regaining life and spirits, and not with any object of lecturing or banqueting. Ever since 1882 I have been frequently importuned by people starting on trips to England for letters of introduction to Spencer. These I have invariably refused, for the reason that I appreciated the condition he was in, and I knew that he did not care to entertain visitors. In all his letters since 1882, he speaks of his inability to perform the work which he, in years past, considered a light task. Spencer remained a life-long bachelor. He entertained no peculiar notions relative to matrimonial affairs, and was simply a celibate from choice. I have heard it said, however, that he at one time entertained a more than friendly regard for George Eliot, whom he persuaded to become a novelist, but I have never been able to find the slightest foundation for the story.

FACE ANALYZED

"I think his whole object in remaining single was that he was afraid that marriage might operate as a check or hindrance on the career he had mapped out for himself.

"You ask what my impressions were of the great English philosopher. I'm afraid that is easier asked for than done. I have always entertained a peculiar notion with regard to Herbert Spencer, whose character, was one of the most complex, I think, I ever encountered. To illustrate, let us take this picture of Spencer (here Prof. Ward opened the first volume of Spencer's "Synthetic Philosophy," and turned to the frontispiece, bearing a photogravure of the distinguished sage), and draw a line horizontally through his face, so that a part of his nose, his mouth, and his chin are on the lower side of the line, and the upper part of the nose, eyes, eyebrows, and forehead are on the upper. Very well; now cover the part above the line with a sheet of paper, so that all that remains visible of the features of this great man are his nose, chin, and mouth. (Here Dr. Ward shaded the upper part of Spencer's face with a sheet of paper.) Now, then, let us contemplate the lower part of the features, and try to form some judgment of his character by them alone. Here we have the features of the typical Scotch bigot; the man who said he was open to conviction, but would like to see the man who could convince him. The sharp nose, the tightly drawn lips, the firm, hard, even brutal, lines around the mouth; and the belligerent and bulldog chin. Here are the features of the man who would kill another for the crime of differing with him on points of religion, the face of the uncompromising, undaunted, unterrified Scotch bigot. The character lined in those features is that almost of a savage.

MENTAL DEVELOPMENT

"Very well; having reached this conclusion in regard to his character, let us now shift the paper, so that only the upper part of the face is shown. Note the enormous mental development, the massive brain, the kindly eyes, set widely apart, the arched eyebrows, and the shapely contour of the bridge of the nose, and the low cheek bones. Here we have the undoubted marks of the sage, the philosopher, the gentleman, the man of the world, and of the liberal. Combine the two (at this juncture Prof. Ward lifted the paper, so that the massive forehead, as well as the brutal chin, the high, arched brows, and the savage mouth; in short, every lineament of his wonderful countenance, was visible), and you have Herbert Spencer, man and philosopher. It is a face intensely English-Scotch, if I may be allowed the use of such a term, a strange blending of the civilized and barbaric.

"I shall, therefore, endeavor to answer your second question, as to my views concerning the effect of Herbert Spencer's philosophy on the civilized world, using the foregoing by way of illustration. Whatever criticisms there may be of Herbert Spencer's political philosophy, or whatever fault we may find with his views concerning the origin and functions of government and religion, the one great fact remains that the chief value of the work which he accomplished through the medium of his 'Synthetic Philosophy' was the establishment of the doctrine of evolution, discovered and first announced by Lamarck. For this one service we can readily forgive the shortcomings of his system.

APOSTLE OF SCIENCE

"To-day, no one thinks of disputing the truth of evolution, and it has become a doctrine as thoroughly settled and grounded, as that of the Monroe doctrine in politics, or the law of possession in jurisprudence. No modern preacher, unless he wanted to make himself ridiculous, would think of attacking the doctrine of evolution which has become a settled fact and law throughout the civilized world. But such was not always the case. Therefore, the men who discovered, and who were courageous enough to boldly preach the new doctrine of evolution, opposed by the church, the universities, and the nobility of England, were of necessity obliged to combine the qualities of the philosopher and fighting bigot in order to gain the day.

"In Herbert Spencer these qualities were blended in the proper proportions and he became, in consequence, the great apostle of science of the nineteenth century. Had he been just a little more of the bigot, and a little less of the philosopher, or vice versa, he would, I believe, have failed in the execution of the great mission which he has alone performed so nobly and so well. Spencer had a hard up-hill fight for it. Although the fact is not generally known, it is true that even at this day, the English universities do not teach Herbert Spencer.

PREJUDICE OF ENGLISH

"The English universities have ever been ultra-conservative, and even the old-fashioned doctrine prevails in them, that new and advanced scientific ideas and doctrines are things which should be held in check; that they are dangerous, unless under strict control. About a year ago one of the English universities relented and offered its chair of science to Herbert Spencer. The offer roused all his old-time fighting spirit, and, in his reply declining the honor, he stated that had the university offered him this place during his younger days, when he was struggling to complete his system, then they might have accomplished some good for science, but that to offer him such a place when old and worn out, was a thing which he particularly resented.

"Spencer had several warm friends on this side of the water, notably Dr. Edward L. Youmans, Dr. G. Brown Goode, of the Smithsonian Institution, both of whom are dead; and George Iles, proprietor of the Park Avenue Hotel, New York. That feature of Washington which left the most lasting impression on the mind of the great philosopher was the Patent Office. He visited it, spent nearly a whole forenoon there, and when he learned that part of the exhibits, the most interesting part, in fact, were in the National Museum, he and Mr. Lott came over and were shown through by Dr. G. Brown Goode. He left here for the Western cities, where, after a three months' tour, he returned to New York, from which port he sailed for Liverpool on November 11, 1893 on the White Star steamship Germanic. Before he did so, however, he was tendered a farewell banquet at Delmonico's on November 9, which I had the honor of attending, and at which I was invited to speak, only, owing to the fact that Henry Ward Beecher and several others monopolized so much of the time with their remarks, myself, Prof. Youmans, and a number of other men of science, were denied the opportunity."

January 16, 1904—Ætat. 62.

494. Administrative Report for the Year ending June 30, 1903

History.—Written May 31, 1903, and left to be copied and sent in at the end of June, as I was to sail for Europe on June 6. The volume reached me January 16, 1904.

Twenty-fourth Annual Report of the United States Geological Survey, 1902–3, Washington, 1903, p. 75.

January 22, 1904—Ætat. 62.

495. [Note on the Degree of Officer of Public Instruction of France, Conferred]

History.—Written January 12, 1904. On December 21, 1903, I received a letter from Dr. René Worms, dated Paris, December 10, 1903, of which the following is the opening paragraph:

"Sur ma proposition M. le Ministre de l'instruction publique a bien voulu vous conférer le grade le plus élevé de notre ordre académique, celui d'officier de l'instruction publique. D'ordinaire il n'est accordé qu' aux personnes qui sont depuis cinq ans déjà, titulaires de grade d'officier d'académie. En vous le conférant directement et sans ce préliminaire, M. le Ministre a voulu marquer la haute estime en laquelle il tient vos travaux scientifiques et sociologiques."

The diploma was enclosed in the letter. I wrote asking whether any duties were connected with this honor, and he replied that there were none, that it was exclusively an honor. Speaking of these "palmes académiques," he said:

> "leur nom vient de ce que la France est divisée pour l'instruction publique en 17 régions, appelées académies, dans chacune desquelles se trouvent une Université, plusieurs lycées et collèges, de nombreuses écoles, etc. Vous avez reçu immédiatement, dans cet ordre, le grade le plus élevé."

Science, New Series, New York, Vol. XIX, No. 473, Jan. 22, 1904, p. 156.

The French minister of public instruction and fine arts has conferred the degree of officer of public instruction upon Dr. Lester F. Ward for his scientific and sociological works. This highest degree of the academic order is usually only conferred on persons who have for five years held the degree of officer of the academy.

March 4, 1904—Ætat. 62.

496. Review of The Positive Philosophy of Auguste Comte. By L. Lévy-Bruhl. Authorized translation, to which is prefixed an Introduction by Frederic Harrison. New York, G. P. Putnam's Sons. 1903. Pp. xiv + 363. 8°

History.—Written January 16–28, 1904. On November 28, 1903, I received the book for review from Dr. Cattell. I commenced reading it at once and finished it on December 19. Various causes delayed the review. It was sent to *Science* on January 29, 1904, and proof was read on February 10. I find attached to the manuscript of this review, and no doubt intended to be inserted at the end of the first paragraph on page 377, a sentence, marked "not used." It is also written at that point on the duplicate galley slip, which was sent me, showing that it was an afterthought of mine, and that I intended to insert it in the proof. Whether I actually did insert it and it was not printed, or whether I thought better of it and did not write it on the proof that was returned, I am quite unable to say. The following is the sentence:

The *Politique Positive* is not, however, wholly worthless. Like a ruined temple of the past, whose columns and capitals, friezes and frescoes, still remain and reveal the hand of a master, its pages are full of the noblest thought and the highest ideals which have a value of their own independently of the framework in which they are set.

The "plagiarism" referred to in the next paragraph, without giving names or circumstances, may now be further described, because the plagiarist in question is no longer living, and the same is true of the other quite innocent person most closely connected with the affair. The Scientific Societies of Washington all had the rule of sending out to members notices of meetings with the programs attached. On May 18, or 19, 1882, I received my notice of the 218th meeting of the Philosophical Society of Washington, to be held on May 20, the program consisting of a communication *On the Philosophical Order of the Sciences* by Prof. Charles W. Shields of Princeton College, who had been invited by Dr. J. C. Welling to come and address the Society on that subject. I think the notice gave the title as *The Classification of the Sciences*, but the above is the title given in the Society's *Bulletin*, Vol. V, p. 105. I was much interested at that time in the classification of the sciences, and was perfectly familiar with the systems of Comte and Spencer. I had no idea that Professor Shields, coming, as he did, from Princeton, would mention Comte's hierarchy, but I thought he might deal somewhat with Spencer's system. I took with me to the meeting a copy of Spencer's little book on the *Classification of the Sciences*. As all this happened more than a quarter of a century ago, I would not like to attempt to describe it from memory, but fortunately I always kept a diary, and before retiring that night I wrote it all out and now have the account before me. The reader is asked to excuse the abbreviated style of a personal diary not meant for other eyes than my own, and also for what would be, if said anywhere else, an inexcusable display of egotism. This is the record:

"Went to the Phil. Soc. Prof. Shields of Princeton, a guest of Dr. Welling, gave us an elaborate paper on the Classification of the Sciences, which was really nothing but Comte's hierarchy clothed up but scarcely improved. I took Spencer's little book along, but it was of no use. At the close of the paper, after the chair had announced that remarks were in order, there was a long pause, no one seeming to have the courage to say anything. At length

I got up and made probably the very best extemporaneous speech I ever made. Being perfectly *au fait* with the subject, I showed them that the system presented was essentially Comte's, pointed out Spencer's inconsistency, and made two telling criticisms on the paper itself. The speech was very warmly received and brought out Powell, Antisell, and others. At the close Dr. Welling congratulated me warmly and asked the privilege of introducing me to Prof. Shields. Dr. Baker and I took a long walk [after the meeting]. He thought my speech was a magnificent take-down."

I do not say here that Comte's name was not mentioned by the speaker, but such was the fact, and we were left to infer that the system propounded was that of Prof. Charles W. Shields!

After this review appeared I received several letters criticizing my failure to mention Harriet Martineau's condensation of the *Positive Philosophy*, the writers, with one exception, supposing that that was a translation. The omission was intentional, but was a mistake. I certainly should have mentioned it and pointed out how far it falls short of a translation. That is not to say that it is not an admirable piece of work, but it has probably prevented a full translation from being made.

Ibid., No. 479, March 4, 1904, pp. 376–380.

March 18, 1904—Ætat. 62.

497. Monogenism and Polygenism

History.—This is merely the Secretary's minute of a forty-minute paper read from notes before the Anthropological Society of Washington on February 9, 1904. The Secretary, Mr. Walter Hough, brought me his report on February 23, and I revised and largely rewrote it for him. In the notice of the meeting these words were misspelled "Monogeism" and "Polygeism." I had handed the title on February 1, to Dr. Lamb, who met me on the street, told me he had been elected president of the Society, and asked me to read a paper. As this subject was discussed by Gumplowicz and myself on the occasion of my visit to him in August, 1903, I obtained and sent him a copy of *Science* containing this note.

Ibid., No. 481, March 18, 1904, p. 458.

March 31, 1904—Ætat. 62.

498. Herbert Spencer's Sociology

History.—Written February 10–15, 1904. On December 19, 1903, I received a letter from Mr. Hamilton Holt, Managing Editor of the *Independent*, saying that the

> "reviewer who reviewed your book on *Pure Sociology* tells us that we cannot afford not to have an article from you on Herbert Spencer as a sociologist."

They had printed one by Prof. Franklin H. Giddings on Spencer's rank as a philosopher, which also gave him high rank as a sociologist, and he added:

> "Knowing the different view-point you take in judging sociological data and particularly the wide divergence between yourself and Mr. Spencer on social legislation, we should deem it of very great importance to publish an article from your pen criticizing Spencer's attitude regarding social legislation."

I had other articles in hand, had to go to New Orleans holiday week to attend the meeting of the American Economic Association, and was busy with work on the *Century Dictionary*, and I did not see how I could do this extra piece of work. I must have written something to that effect, and there was some correspondence. But late in January, 1904, I thought I saw my way clear to write it and so informed Mr. Holt. On February 5, he renewed his request, and I made quick work of it, blocking it out first in pencil and then copying it. Proof was read on March 5, and a copy of the number containing it reached me on March 30. I had ordered 100 handsome reprints which I received on April 8.

Incidental reference has been made (supra, No. 497) to my first visit to Gratz in 1903 and the all-day discussion I had with Dr. Ludwig Gumplowicz on various sociological questions. I had no idea that anything would come of it, but about a year later I received from him some copies of *Die Zeit*, a well-known weekly journal published in Vienna, bearing date August 20, 1904 (Vol. XL, No. 516), containing on pages 86–90, an extended article by Gumplowicz, the title of which consisted simply of my name. In this he gave a full account of the interview in question, in the happiest vein and with the most admirable temper, admitting the force of my arguments against his polygenisms and other "isms" that I tried to induce him to renounce. In the meantime the present article had appeared and I had sent it to him. He must have been much struck with it, for he had translated it into German, and inserted it entire into this article in *Die Zeit* (pp. 87–88). Not content with this, in writing his great work: *Geschichte der Staatstheorien*, which appeared a year later, he devoted a section to me (§119, pp. 418–432), and in this he again inserts his translation of this article (pp. 423–428). I sent a copy of *Die Zeit* containing Gumplowicz's article to Dr. Small and he translated all except my article and published it in the *American Journal of Sociology* for March, 1905 (Vol. X, No. 5, pp. 643–653), with the title: *An Austrian Appreciation of Lester F. Ward*, of which he kindly sent me numerous reprints.

It has already been stated that when Mr. Unoské Wakamiya called on me February 17, 1906, I handed him one of these reprints, and he says in his letter of May 7, 1909, that he had translated and published it. On March 29, 1909, I received from Mr. Kaneko a copy of the Japanese translation as it appeared in the *Tokyo Economist*.

To his translation Mr. Wakamiya affixed a descriptive note which he asked Mr. Kaneko to translate for me. He did so and sent it to me on March 26, 1909. I give it entire:

> The following article appeared in the New York *Independent* of March 3, 1904, immediately after the death of Herbert Spencer. The author wrote this article by special request of the editor of the *Independent*. It is interesting to observe the skilfulness of the author in drawing his sociocratic conclusion and using the hypothesis which Spencer presented in his individualistic biology. The importance of the article is proved by the fact that Gumplowicz had a chapter entitled "Ward vs. Spencer" in his *Geschichte der Staatstheorien*, giving nearly the whole of this article by Ward. In Japan to-day we often see students use the term sociology, but their sociology has nothing to do with the betterment of society. It is simply a "Statical Sociology" and is not very far from that of Spencer. Constructive sociology really begins with that of Lester F.

Ward. Those who are not satisfied with so-called social polity, which is nothing but an empirical method of curing diseases, come to Ward and master Ward. Those who want to know the sociology that is founded on scientific truth and aims at real social betterment, learn Ward by heart. This article probably represents the essential points of his system of sociology in the clearest and simplest words, and consequently deserves the careful attention of readers.

U. WAKAMIYA.

This paper was reprinted entire in the *Plebs* Magazine for April, 1911.

The Independent, New York, Vol. LVI, No. 2887, March 31, 1904, pp. 730–734; Reprint with cover and title-page, New York, 1904, pp. 730–734; The Plebs Magazine, Oxford, Vol. III, No. 3, April, 1911, pp. 52–58.

[Professor Ward's sociology differs fundamentally from that of Spencer. From the time of his first work on the subject, "Dynamic Sociology" (1883), to his latest, "Pure Sociology" (1903), he has vigorously combated the biological and in a sense fatalistic conception of the Spencerian school. In the emphasis which the Spencerians have put upon the purely biological factors of human evolution they have slighted or minimized the factors of intelligence. To such extremes has this view been carried that social regulation has been denounced as an interference with evolution—as tho an alien force could be intruded into the cosmic process. It has been the work of Professor Ward to keep constantly before the world the truth that human intelligence, far from adapting itself to its environment, radically alters that environment, and that consciousness and will are as truly a part of the evolutionary process as are chemical reaction and gravitation.—EDITOR.]

C'est le privilège du vrai génie, et surtout du génie qui ouvre une carrière, de faire impunément de grandes fautes.—*Voltaire.*

"NIHIL de mortuis nisi bonum." In these days, when science mourns, no one covets the task of pointing out defects. If Herbert Spencer did not create sociology, he at least raised it into life and started it on its grand career. It required courage to do this and to embody it in a great scientific system on an equal footing with biology, psychology and ethics at a time when others passed it by and disdained to speak its name. This brave act will always be regarded as more than atoning for any shortcomings that the most critical will ever find in Herbert Spencer's sociology. As one of those who have freely criticised that part of his vast scheme during his lifetime, the present writer feels it incumbent frankly to avow that nothing he could say in disparagement of certain aspects of Spencer's treatment of the subject appreciably diminishes the debt of gratitude which he, in common with all lovers of truth, acknowledges to Herbert Spencer for the three monumental volumes in which he has unfolded that science.

But the saying is too trite to need repeating that there is always danger of resting any case upon authority, however great, and that the only condition to progress in any science or in any field of inquiry is fearless and independent scrutiny of the basic doctrines of even the greatest masters. It is, therefore, no derogation from the magnitude of Spencer's achievement to say that, like all things human, it has its defects. In the present case it is perhaps better to say, that it has one defect, for by the side of this one all the others are dwarfed into insignificance. And but for the weighty and vital character of this defect it would be unworthy of any one at a time like the present to point it out, and thus break the even flow of just and sincere praise for one who has made an epoch and has now laid down his pen.

How shall we formulate this one salient deficiency in Herbert Spencer's sociology? It may sound too dramatic to say that it consists in ignoring the human mind as a factor in sociology. True, his system embraces two volumes on psychology. Nevertheless I make bold to affirm not only that he did not base his sociology upon his psychology, but that his psychology is of a kind such that sociology could not be based upon it. Written before the biology and transferred to a position between that and the sociology, where, of course, it should stand, it is, nevertheless, as completely isolated as if it formed no part of the Synthetic Philosophy. The sociology, great as are its intrinsic merits, does not represent a science like other sciences, upon which man can lay hold and use as an instrument for his own advancement. Every other science rests upon a body of uniform laws which have been discovered by investigation, and which, as soon and as fast as discovered, can be put to immediate use in furthering the interests of life and ameliorating the condition of mankind. The science of sociology as taught by Spencer is a complete exception in this respect. Its laws are not pointed out, and there is not only no intimation that if there are social laws they may be utilized to human advantage, but there is a distinct implication, repeatedly expressed, that no such use can be made of them.

In Spencer's psychology spare allusion is made to the most fundamental and essential of the intellectual faculties, the faculty of invention. This is the faculty that has the chief value in sociology. It is the one that has produced nearly all the effects that distinguish man from an animal. But for it he could never have migrated and peopled the earth. It is the basis of all the arts. It underlies all discovery in science. It has accomplished the whole of what is called material civilization. It has done this by applying the known laws of nature to the uses of man. The various sciences, one after another, as fast as established by the discovery of their laws, have thus been put to practical service. There is no law of nature which cannot be made available for such purposes to a greater or less extent. It is the essence of a science that it shall explain certain invariable laws governing the phenomena with which it deals. All true sciences are of that character. If sociology is really a science it must also possess this character. And as man has been able to make practical use of every other science, it must follow that when social laws are really known and a social science is established he will be able to make a practical use of it. This in Spencer's sociology is at least impliedly denied, and in his other works it is expressly and vehemently denied.

To every science there corresponds an art. If there is a social science there must be a social art. That there is such an art no one can doubt, but thus far, it must be frankly confessed, it has remained chiefly an empirical art. In this respect it does not differ from all other arts. All have their empirical stage before they reach their scientific stage. But the empirical arts have all been useful, and the social art, even in its empirical stage, has been the most useful of all, since it has been the condition to the development of all the other arts—nay, to the very existence of society itself. But just as the usefulness of all other arts has been enormously increased by scientific discovery, so the usefulness of the social art will be increased, and in quite as great proportion, by the discovery and application of the laws and principles underlying social phenomena. It is not necessary to point out what all this is to consist in. I have attempted this on numerous occasions and gone as far as possible with the light we possess. But the essential thing is to recognize social phenomena as a field for scientific discovery and for the exercise of the inventive faculty precisely as in other departments of science and art. This class of scientific research once recognized and entered upon, the possible directions that it shall take, the methods and the technic will soon reveal themselves. If there are social laws and social forces, work in the social field will differ from that in other fields only in the nature of these laws and forces. Just as invention in the physical world consists in directing physical forces into channels of human advantage, so invention in the social world must consist in directing the social forces into such channels. The empirical social art seeks to drive men to do what is supposed to be for the interest of society. The inventor never seeks to coerce natural forces. By means of appropriate apparatus devised by his ingenuity he causes or *induces* them spontaneously to flow in the desired channels. It must be so with the social inventor, and social invention, once seriously undertaken, will speedily do away with all mandatory, prohibitory and penal legislation, and inaugurate an era of scientific, or *attractive* legislation, which will make obedience to law the form of action that the individual most desires, thus rendering the operations of society automatic and spontaneous.

Of all this we find absolutely nothing in any of the writings of Herbert Spencer.

There is another point of view from which we may contemplate Herbert Spencer's sociology. In his biology we are taught that organic evolution takes place through the joint action of differentiation and integration. Organic progress is measured by the degree to which organs and structures are multiplied to serve the various ends of higher and higher life, and by the degree to which these multiplied structures and organs are then subordinated to the directive influence of a more and more perfect nervous system, and ultimately to the absolute control of one supreme directive organ, the perfected brain. It is these two conditions which constitute respectively organic differentiation and organic integration.

Mr. Spencer early espoused the doctrine that human society constitutes an organism analogous in many respects to the organisms of which the world of life is composed. He pointed out these analogies in great numbers and supplied the most convincing arguments for the doctrine that have yet been adduced. But it is noteworthy that he generally, and no doubt intentionally, avoided, as far as possible, those analogies that relate to the nervous system, altho it is here that the most important ones are to be found. He did, however, say that the function of Parliament was analogous to that of the brain of animals, and it is the prevailing view of those who defend the social organism theory that government is the sociological homolog of the brain.

Certain it is that organic integration is effected solely through the nervous system, and, in the higher organisms, through the brain. The various organs and structures would never spontaneously co-operate in the interest of the whole organism and work together in that perfect harmony necessary to carry on the functions of life without an organized nervous system to give and execute the commands of the creature. Mr. Spencer laid great stress on social differentiation, but was almost silent as to social integration. But what is social integration? Evidently it is some co-ordinating system that regulates the manifold organs of society and requires them to work in harmony for the good of the whole. And what can this be but the power, however constituted and by whatever name called, that every society, however undeveloped, possesses, and which is the agent and exponent of the society? Sociologists prefer to speak of collective action, or of the action of society itself, rather than to use the narrower, less correct, and more or less objectionable word government. That term is apt to be taken to mean the particular persons whom society at any given time has selected as its agents to execute its will and conduct its affairs. These persons are nothing but instruments and of no significance from the sociological point of view. The action taken in any case is that of society acting as an integrated unit, in the same sense as the acts of an animal or a man are those of the complete organism under the control of a nervous system presided over by a supreme central ganglion or brain.

Is the analogy, then, to stop with differentiation only? Is the social organism nothing but a complex mass of highly differentiated organs and structures without any co-ordinating and controlling system capable of making them work to some prescribed end and co-operate in carrying on the functions of society? Such is the conclusion to be drawn from Herbert Spencer's sociology.

Mr. Spencer started out in his great career as an avowed and extreme individualist, thoroughly imbued with the doctrines of the economists of his day. Individualism was then and is still taken to mean the opposite of collectivism. But, properly viewed, it is nothing of the kind. It can be successfully shown that there is nothing contradictory in the two doctrines, and that true individualism is not only consistent with true collectivism, but can, in fact, only be attained by means of it. This may seem paradoxical, but it will seem still more so when I say that the proof of it has been chiefly supplied by Mr. Spencer himself.

In defending the doctrine of the social organism he introduced certain qualifications. He enumerated first the respects in which a society resembles an organism, and, secondly, those in which it differs from one. Altho he reduced these latter to a minimum, and often showed that they were more apparent than real, there remained one which he saw to be fundamental. He said:

> "The last and perhaps the most important distinction is that while in the body of an animal only a special tissue is endowed with feeling, in society all the members are endowed with feeling. . . . It is well that the lives of all parts of an animal should be merged in the life of the whole, because the whole has a corporate consciousness capable of happiness or misery. But it is not so with a society, since its living units do not and cannot lose individual consciousness, and since the community, as a whole, has no corporate consciousness. And this is the everlasting reason why the welfare of citizens cannot rightly be sacrificed to some supposed benefit of the state, but why, on the other hand, the state is to be maintained for the benefit of citizens. The corporate life must here be subservient to the lives of the parts, instead of the lives of the parts being subservient to the corporate life."

Thus we see that it was upon this one important respect in which society differs from an organism that Mr. Spencer justified his individualism and maintained that his attitude of hostility to collective action was in harmony with his general scheme of philosophy. In this he was certainly mistaken, and, what is more, it is this undeniable fact that the individual alone is capable of enjoyment and suffering that constitutes the chief argument for striving to attain the maximum social integration.

According to the Lamarckian law, which Mr. Spencer fully accepted, it is function that creates organs. Whatever organs, structures or parts an organism acquires, they have all been developed in response to a demand growing out of the needs of the organism. The consequence is that all development, whether organic or social, always is and must necessarily be in the direction of some specific advantage to be derived therefrom. And, conversely, whatever is demanded as such an advantage will ultimately be supplied by the development of the structure, organ, or part that is adapted to secure it. Now, as he says, it was manifestly to the advantage of the organism that the manifold parts should come under the complete dominion of the whole organism. Any individual liberty on the part of the various organs and structures would quickly entail the destruction of the organism with all its parts. And all because it is the organism that is sensitive and conscious, while the parts may be regarded as, relatively at least, insensible and unconscious.

All this is reversed in a society, and for the very reason that Mr. Spencer gives—viz., that here it is the parts that are sensitive and conscious, while the society as such is unconscious and incapable of either enjoyment or suffering. This it is that explains the difference between social integration and organic integration. The former, according to the Lamarckian law, is directed exclusively toward securing the interests of the parts—*i. e.*, of individuals. It is, and must necessarily be, introduced for this purpose. Nothing can originate, either in the organic or the superorganic world, which is not advantageous. To conceive of the origin of a disadvantageous organ, or structure, or institution, is to misunderstand the first principle of evolution. The end is the cause of the means, which arises solely for the sake of the end. The end is always the *good* of something. The idea of a good involves that of the capacity to feel. In a word, it implies a sentient being. An organism is such a being, but a cell or a gland is not. An individual man is such a being, but a state or a society is not. It follows that organic development must be in the direction of securing the interest of the organism and not of its parts, while social development must be in the direction of securing the interest of the individual and not of society as such. The general means employed in both cases has been integration but organic and social integration are unlike in this respect.

Just as in the organic world structures have been the means employed in securing the end—the good of the organism—so in the superorganic world society has been the means employed in securing the end—the good of the individual. This social integration, which is the scientific expression for collectivism, is the only means by which the freedom and happiness of the individual can be secured. Primarily it was the only means by which the human race could protect itself from hostile influences and continue to exist. And now, as always, it is the only means of protecting individuals from the egoistic domination of other individuals. Without a co-ordinating and restraining power to regulate the conduct of individuals toward one another and prevent the wholesale exploitation of the weak by the strong, liberty and happiness would be impossible. The highest aim of true individualism is the maximum individual liberty. Social integration somewhat restricts the individual's freedom, but this restriction is as nothing compared to that which other individuals would cause in its absence. Even Mr. Spencer's "equal freedom" can only be attained through such collective restraint as shall forbid one individual to interfere with the liberty of another. Within these limits the more complete the social integration the greater the real and legitimate liberty of every individual.

The existing "social unrest," of which we are hearing so much, is due in the main to the imperfect state of social integration at which the world has arrived, and its sole remedy must be through more and more complete integration. The present social movement is wholly in this direction. Mr. Spencer saw the movement, but he misinterpreted it. He saw in it "the coming slavery," instead of the coming liberation of mankind. He imagined that it was morbid, abnormal and temporary, whereas it is perfectly healthy, normal, and destined to continue. He did not perceive that the fundamental distinction which he so clearly pointed out between the animal and the social organism necessarily reverses the direction of social evolution and causes it to work for the good of the individual.

The movement toward collectivism, which no one with his eyes open can fail to see taking place in spite of all that the philosophers may say, is really a true social evolution, proceeding on natural principles, and aiming at the same end as all other forms of social progress—the good of mankind. It differs from organic evolution only in the fact that it seeks the good of the parts instead of the whole, of individuals instead of society considered as something to be benefited. If Mr. Spencer had seen this he might have made his sociology not only symmetrical in itself but harmonious with his entire scheme of philosophy, of which it would have become the natural culmination and the true crown.

499. Review of Where did Life begin? A Brief Inquiry as to the Probable Place of Beginning and the Natural Courses of Migration therefrom of the Flora and Fauna of the Earth. A Monograph. By Gilbert Hilton Scribner. New Edition. New York: Charles Scribner's Sons, 1903

History.—Written February 24–26, 1904. On February 19, 1904, Mr. F. W. Hodge, Editor of the *American Anthropologist*, sent me the book with the request that I review it. I had somehow missed the first edition, and was glad of an opportunity to read the book, which I did at once, and finished it on February 21. I had long been interested in the theory of the polar origin of life and had collected some references, but when I now undertook to extend them I was surprised to find how rare they were. I spent two days in an almost vain search for them. I then sat down and wrote the review and sent it to Mr. Hodge on February 27. Proof was received on March 15, and the number of the *Anthrolopogist* containing it arrived on April 26. Mr. Hodge had 100 extras struck off for me which he sent me on May 5. I could not distribute half of them, and still have a large number. I sent one to Mr. Scribner, and on May 19, I received from him a copy of his book neatly inscribed to me.

The American Anthropologist, New York, Vol. VI, No. 1, January–March, 1904, pp. 151–152.

June 10, 1904—Ætat. 62.

500. Herbert Spencer's Autobiography*

History.—Written May 23–30, 1904. On January 11, 1904, I received a letter from Dr. Cattell asking whether I would be willing to write a memorial article on Herbert Spencer for the *Popular Science Monthly*, and also a review of his *Autobiography*, which had just emerged from the press, but of which he had not yet received a copy. I answered his letter the next day and expressed a willingness to review the *Autobiography* when I received it. On April 24, I saw a review of it in a newspaper and I wrote and told Dr. Cattell that it was out. He wrote me on the 28th that he had ordered the publishers to send me a set, and it came on May 4. I commenced reading it immediately, giving it precedence over all other unofficial work, and by May 18, I had read both volumes. I then spent four evenings going through them and making slip notes of all the important points, and arranged these in alphabetical order. I first made a rough pencil draft of the review, which was finished on May 28, and I copied it at once and sent the final draft to Dr. Cattell on the 30th. Proof was read on June 6, and copies reached me on the day of date. I intended it as a review and wrote it as such, but it was made the first and leading article of that number of *Science*.

Science, New Series, New York, Vol. XIX, No. 493, June 10, 1904, pp. 873–879.

THE autobiography of a great man, the publication of which during his lifetime is expressly interdicted by him, unavoidably raises the question as to the possibility of disinterested action. Mr. Spencer has, indeed, in his "Autobiography" discussed the motives that prompted his work, and has shown that egoism and altruism are inextricably mixed in the composition of these motives. But he speaks only of his philosophical works, all of which appeared during his lifetime, and in which he may, therefore, be supposed to have a personal interest. But here is a work of no mean proportions, in which he knew he could take no interest after it appeared. In many cases the motive may be explained by the belief on the part of the authors that they will continue to exist and remain cognizant of all that is to take place, and will, therefore, know just what the effect of their action is to be upon the world at large. But no such motive can be alleged in the present case, for he himself says: "as I have no belief in anything to be gained in another world, it can not be otherworldliness that moved me"; and again: "with death there lapses both the consciousness of existence and the consciousness of having existed." This is not the place to discuss such a question, but in the minds of many it can not be suppressed.

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* Two volumes. New York, D. Appleton & Company, 1904, 8°.

The "Autobiography" of Herbert Spencer must not be regarded as a mere pastime and incidental episode in his career, but as an integral part of his life work. Whereas his other works constitute his philosophy of nature, his "Autobiography" constitutes his philosophy of life. It is a large work, seriously written, costing him years of labor. It was not written after his main work was done as a closing retrospect to his laborious life, but was executed in the midst of his busiest days, while he was hard at work on his "Synthetic Philosophy." It was begun, he tells us, in May, 1875, *i. e.*, while he was writing the first volume of his "Principles of Sociology," and the main portion of it was finished on his sixty-ninth birthday, April 27, 1889, or while he was writing the first volume of his "Principles of Ethics." It is true that four years later he wrote some "Reflections," which occupy the last sixty pages of the "Autobiography," in which some of the events of that period are alluded to, but this is not, like the rest, a chronological record. But even if we place the conclusion of this work at the year 1893, which is the date of the second volume of the "Principles of Ethics," we find that it ended before the appearance of either of the last two volumes of the "Principles of Sociology," although parts of the second volume had been published. The third volume bears date, 1897. There were then still four years of activity after the last word of the "Autobiography" had been dictated before the conclusion of the "Synthetic Philosophy." He survived his great work six years, and there are evidences that he was by no means idle during that time. In a letter dated May 4, 1897, although he characterizes himself as a "wreck," still he speaks somewhat doubtfully of his ability to complete his "remaining task—revision of the 'Principles of Biology.'" Why he did not bring his "Autobiography" down to some such date, or even later, has not yet been explained.

This work has done the important service of dispelling a large amount of popular error with regard to Herbert Spencer's life and career. The prevailing opinion has been that he was a typically "self-made man." He has been represented as having had to struggle with adversity, and has been held up as a proof of the theory that great abilities are certain to assert themselves whatever the obstacles may be in their path. His life shows that, on the contrary, he was highly favored by circumstances. While of course without his talents his achievements would have been impossible, still, given such talents, there was scarcely any reason why he should not have accomplished great things. He does not himself favor the Galtonian doctrine, but fully recognizes his indebtedness to circumstances. He admits that but for the three legacies that were one after the other left him by his two uncles and his father, he could never have completed his system. But he was even more indebted to the help of influential friends, freely volunteered, and by a whole train of favorable circumstances, fully set forth in his "Autobiography." Indeed, his very environment was sufficient to bring out all that was in him. On intimate terms for the greater part of his life with such men as Huxley, Tyndall, Hooker, Lubbock, Mill, Lewes and Bain, belonging to the same clubs, taking long walks, and having constant discussions with them, the stimulus must have been enormous.

He enters quite elaborately into the question of genealogy, and shows that his ancestors embodied extremely heterogeneous elements, elements, as he maintains, calculated to implant in him most of the characteristics that he possessed. To a groundwork of immemorial English and a little Scotch there was added a strain of the French Huguenot, probably tinctured with Bohemian Hussite protestantism. It must not, however, be supposed that this ancestral heterogeneity rendered him any the less typically English, for the one leading characteristic of the whole Anglo-Saxon race is the complete mixture of all the numerous races—Saxon, Danish, Norman, British, Welsh, Scotch, etc.—that entered into the composition of the later inhabitants of that historic isle.

Herbert Spencer is commonly represented as being the type of a self-educated man. Nothing could be farther from the truth. The son of a professional teacher belonging to a long line of teachers, he was surrounded by educational influences from his very birth. So far from struggling to educate himself, his main efforts as a boy seem to have been to escape from the perpetual drill of the domestic school. His father finally sent him away to be further drilled by his uncle, but it was the same old story, geometry forever. His youthful escapade from this latter educational treadmill is very amusing. Many boys of some pluck, when they imagine themselves ill-treated at home, *run away*, but Spencer, thinking himself overtasked by his uncle, *ran* *home*, from Hinton to Derby, a distance of nearly 150 miles! He admits that it was largely homesickness, and one can compare it to nothing but the way a domestic animal, removed from the spot to which it has become wonted, will seize the first opportunity to go back, regardless of the distance, and guided by that little-known "sense of direction" that some think to be located in the semicircular canals of the ear.

But whatever his treatment may have been, and it certainly was never severe, Herbert Spencer as a boy was always being taught. His education was not sporadic and one-sided, but methodical and all-sided. He is usually represented as wholly ignorant of Greek or Latin and of modern languages. In so far as this is true it was due to his distaste for them, for he complains of being taught them. At that day, before the natural sciences had come to receive the place they now occupy in education, all pupils belonged to one or the other of two classes, those that loved mathematics and hated languages, and those that loved languages and hated mathematics. Spencer belonged to the first of these classes. But he had to learn languages and dead languages at that, and any close observer of his style can see that he did learn them sufficiently to affect his style. It is clear that he always had the derivation of a word in mind when using it, and that he knew enough Greek and Latin to apply their principles to his own language. He seems to have known very little German, but he not only read French but spoke it well enough to act on one occasion as an interpreter.

All that is left, therefore, of the prevailing notion about his education is that he was not university trained. He thought that a great advantage, and never tired of citing proofs that university training spoils a man for all usefulness and fills him with a mass of useless rubbish. Whether he would have done any better or worse had he taken a university course may be a difficult question to answer, but his whole reasoning on the subject is unsound because it is based on the exceptional man and takes no account of the average man. Indeed, his entire philosophy of education is permeated by this vice. His book on education may be said to rest on the assumption that every child has a father or a mother or both capable of properly educating him or her. One has only to look around to see how absurd this assumption is.

Herbert Spencer belonged to the middle class; though not rich, he was by no means poor. He never did manual labor of any kind and none of his ancestors at all recent belonged to the laboring classes. He explains the smallness of his hands by this fact. He never knew what it was to be in want or to fear that he might come to want. The only work of a bread-winning kind that he ever did was while serving as a civil engineer in the construction of certain railroads. This occupied nine years of his life (1837–46), from his seventeenth to his twenty-sixth year. The several positions that he held during this time were not sought, but were offered to him, generally, as he admits, through the influence of his friends rather than from any superiority of his own in the business. More than once he gave up a good position and returned home for awhile. But his father's latch-string was always out and he was welcomed back whatever might be the cause of his coming. He alleges as one reason for not holding his positions longer, his "lack of tact in dealing with men, especially superiors," and says: "Advancement depends rather on pleasing those in authority than on intrinsic fitness. . . . Never did it enter into my thoughts to ingratiate myself with those above me. Rather I have ever been apt, by criticisms and outspoken differences of opinion, to give offense." In other words, he was no toady, and never cared to "crook the pregnant hinges of the knee where thrift may follow fawning." But he was never removed from any position. He always voluntarily quit work, usually with the regret of his employers.

The only other period of his life that he was subject, even nominally, to the will of a superior was during the five years (1848–53) that he was subeditor of the *Economist*, and this position he also voluntarily relinquished. This was an easy position and left him much leisure time, as may be judged from the fact that during this period he wrote his first book, "Social Statics." It was to be hoped that in his "Autobiography" he would give a full explanation of how he came to choose the title "Social Statics." He does, indeed, discuss a number of titles that had occurred to him, but leaves it to be assumed that the one finally adopted was originally his own. To find the true explanation it is necessary to go to the revised edition of that work published in 1892, where in a footnote to page 233 he says he met with the phrase in Mill's "Political Economy," Mill himself crediting it to another writer, which other writer, though Spencer did not know it, was Auguste Comte. It thus happens that, notwithstanding his strenuous efforts to disclaim all influence of Comte, three of the leading terms of his philosophy, *social statics*, *sociology* and *altruism*, were Comtean terms.

After leaving the *Economist* he devoted himself for a time to article writing, which yielded him some revenue, though scarcely a livelihood, but which had the advantage of enabling him, as Nietzsche would say, to get rid of his thoughts. Instead, however, of getting rid of them, he found them taking complete possession of him. In fact, the very next year (1854) he commenced writing his "Principles of Psychology," which he finished within a year, and the work actually appeared in 1855. But even this, so far from satisfying him, served only the more completely to open up the vista of his future, and although he characterized the next two years as "idle," before the end of 1857 a great system of philosophy had taken shape in his mind. His first rough draft of its main heads was made and dated January 6, 1858. Two years later the complete prospectus was issued, and this was adhered to in most particulars during the subsequent thirty-seven years of its execution.

He had now made his plans known to all his friends and they had unanimously encouraged him to proceed. The great obstacle was publication, as no publisher would undertake so hazardous a work, and after much discussion and advice it was decided to issue the work in parts by subscription. In one of the appendices to the "Autobiography" appears the list of original subscribers. We may judge of the backing that he had, even at the outset, by the following names that are found among others in that list: John Stuart Mill, Charles Darwin, Thomas Huxley, Sir Charles Lyell, Sir Joseph Hooker, Sir John Herschel, Professor De Morgan, George Henry Lewes, George Eliot, Charles Kingsley, George Grote, Alexander Bain, Henry T. Buckle, Jules Simon.

It is interesting to compare the original draft with the final draft of the prospectus of Mr. Spencer's system. Aside from the difficulty of explaining why he called both parts of Vol. I. ("First Principles") the "Unknowable" in the former, while Part II. in the latter deals with the "Knowable," there is the fact that in the original draft he makes Part III. treat of "Astronomic Evolution" and Part IV. of "Geologic Evolution," these being the "two volumes" that were wholly omitted in the completed system. As this original draft was never before published the world was left practically in the dark as to what these volumes would have contained had they been written. In the explanatory note inserted in the preface to "First Principles" (p. xiv) he simply states that the application of these principles to inorganic nature is omitted, but this gives no intimation as to how this application would have been made. He does, indeed, refer in at least two other places to these omitted volumes ("Principles of Biology," Vol. I., Appendix, pp. 479, 480; "Principles of Sociology," Vol. I., p. 3), and in the second of these he says that one of the volumes would have dealt with "Astrogeny" and the other with "Geogeny." These appear to be the only hints that he gave out on this point, and few readers probably ever noticed them. . But in one of his letters written in 1895 he entered much more fully into this subject and set forth clearly just what his whole system would have been had it been fully written out.*

The rest of the "Autobiography" deals mainly with the execution of this great scheme, which need not be followed out. There are, however, many incidental matters connected with the chief matter, and some not connected with it, that have a special interest. Only a few of these can be mentioned. One of them relates to the reception that Mr. Spencer's books met with at the hands of the public. Nothing certainly is more annoying to a writer on philosophical subjects than the reviews of his books. As Spencer says, "adverse criticisms of utterly unjust kinds frequently pursue the conscientious writer. . . . Careless misstatements and gross misrepresentations continually exasperate him." He finally discovered that reviews do more harm than good. An author is lucky if no attention is paid to his books, for it is far better to be "smothered with silence" than to be wilfully or ignorantly misrepresented. A reviewer who has not the caliber to understand a book, but who must, nevertheless, review it because it is sent to the press, will usually indulge in cheap flings at it and apply to it damaging epithets calculated to deter readers from examining it. If it seems radical or opposed to current ideas it will arouse "offended prejudices" or call down the *odium theologicum*. Everybody knows how Darwin's works were treated by the religious press. Then there is the subsidized press, which maintains a strict censorship over the contemporary literature, more effective in some respects than that of despotic governments, and every book that is suspected of being at all "dangerous" is attacked by the leading journals, sometimes with ridicule, sometimes with apparent seriousness, usually by scholarly writers employed for the purpose. Even specialists can always be hired to write books down.

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* See SCIENCE, N. S., Vol. III., February 21, 1896, p. 294; "Pure Sociology," pp. 67–69.

Mr. Spencer found that the sale of his books was being seriously interfered with through hostile reviews. Professor Bain, who was one of the subscribers, told John Stuart Mill that for a long time he did not read "First Principles," saying that the impression gained from notices of it had deterred him. He went on to say that when, subsequently, he read the book he found to his astonishment that the reviews had not given him the remotest conception of its contents. It was, therefore, decided to send no more copies to the press, and this policy was adhered to until near the end of the work. After it had been fairly tested it was found on examining the accounts that the sales had about doubled.

As already remarked, Mr. Spencer was now beholden to no man and could devote all his energies to his great task. But he was destined to become a slave to a worse master than any superior officer. He was to become the victim of an insidious disease, a disease which proved incurable, and which attacked precisely the organ of which he had the greatest need—his brain. It began with insomnia, and was always attended with insomnia, but it soon threatened complete prostration, and from his thirty-fifth year to the end of his life it was one constant struggle for health. But it was not a fatal disease, as he lived well into his eighty-fourth year, and, as he says, it was not a painful disease, and, like most forms of neurasthenia, it did not show in his face, so that people always supposed him younger than he was. But it rendered continuous attention to anything whatever impossible. His work must henceforth be done at short sessions with long intervals of rest. There were sometimes days, weeks and even months that he could do nothing. In the pursuit of health he traveled much and resorted to all forms of amusement. Fishing was his favorite pastime, but he often took long pedestrian journeys.

He must have been a very poor observer. It would seem that he had subordinated and practically sacrificed his perceptive to his reflective faculties. With even the little dips into entomology, botany and geology that he had made in his early life one would suppose that he would have seen more in the world. But he rarely mentions any object in natural history. It is very disappointing to read his account of walks, for example, round the Isle of Wight. He does, indeed, mention the chalk, but he never mentions the far more interesting Wealden formation, and seems to have had no idea of the geology of that island. It was the same with his visits to the Yorkshire coast and other places celebrated for their geological interest. But he observed men and human operations, and usually criticizes everything severely. Nothing in art, ancient or modern, came up to his ideal.

Herbert Spencer, as all know, never married, and it seems certain that his celibacy was the result of a reasoned resolve to let nothing interfere with his main purpose. But it is evident from reading his "Autobiography" that he was not lacking in any of the qualities that would have made family life successful. He often alludes to it as a good that he was compelled to forego. His views of women were of the most enlightened kind, and the ideal of marriage that he sets forth in a letter to a friend about to marry is as perfect and noble as it is possible to conceive of. There are doubtless many readers for whom the most interesting part of his "Autobiography" will be that which treats of his relations with George Eliot, although, so far as can be judged either from this work or from the "Life and Letters of George Eliot," these relations were never in any sense sentimental. But they were certainly much more intimate and more prolonged than any of her letters would lead us to suppose. It is surprising to learn that it was he chiefly who urged her to write fiction, an idea which she could not at first entertain. The "Letters" leave the impression that it was Lewes who played this rôle. Perhaps both equally saw in her this talent before she saw it in herself. It is equally surprising that she should have made Spencer her confidant in the matter of the authorship not only of her first stories but also of Adam Bede. It is to be regretted that she, too, did not write an autobiography.

Such is a hasty glance at a few of the salient points in the "Autobiography" of Herbert Spencer. No two persons would select the same points, and no such glance can hope to do justice to the work. Nothing has been said of his inventions, which were numerous but none of them important or successful; of his numerous essays, from his "Proper Sphere of Government" to his "Factors of Organic Evolution"; of his "Descriptive Sociology," that monumental but costly undertaking; of his "cerebral hygiene," which, unlike that of Comte, consisted in reading nothing that he did not agree with, thus warping, as Comte had dwarfed, the growth of ideas; of his more extended travels, including his visit to America, which latter is familiar to us all; nor of his persistent hostility to governmental initiative (*laissez faire*), which formed so prominent a feature in his political philosophy.

With regard to this last it would seem that owing to preconceptions of his youth confirmed during his connection with the *Economist*, he was unduly frightened by the bugbear of collectivism, which is really nothing but social integration, and a necessary part of the very social evolution which he taught. For this must consist, as in both inorganic and organic nature, of differentiation and integration. His inability to perceive this made his system, so broad at its base, a frustum instead of a pyramid.

The "Autobiography" is written in a much more pleasing style than his other works. It shows its author in all the simplicity of true greatness. His life demonstrates that he was a natural product of his time. He lived at the acme of the Victorian age, the grandest epoch in history, and he was directly in touch with all the powerful forces that characterized that epoch. When we take into consideration his own inherent powers we may say in very truth that his life was "a continuous adjustment of internal relations to external relations," and that he was a normal product of the laws of evolution that he expounded.

June 22, 1904—Ætat. 63.

501. [The Rise of the Proletariat]

History.—Written December 23–26, 1903. On April 25, 1903, I received a letter from Prof. E. R. A. Seligman, President of the American Economic Association, asking me to take part in the discussion of a paper by Prof. F. H. Giddings on Sociology and History, to be read before a joint meeting of the American Economic and the American Historical Associations at New Orleans during the holiday week of that year. I replied the next day offering to do so, and heard nothing more of the matter until December 11, when Professor Giddings called on me in Washington, and told me he would read a paper on *Social Causation*. On December 21, I received a copy of Professor Giddings's paper. On December 23, I wrote a rough pencil sketch of the remarks I proposed to make. I still possess that sketch, and it is headed: *Rise of the Proletariat*. I revised it the next day, and on the 26th I had three typewritten copies of it made, one of which I have, and I also have the manuscript copy of Professor Giddings's paper. It was read at the New Orleans meeting on December 31. I had based my remarks entirely on the last part of his paper relating to what he called the "three great social ideals: those, namely, of unity, of liberty, and of equality." The time was limited and Professor Giddings was very deliberate. He really read only the introduction to his paper, and came far short of reaching the part discussed by me. I was down to discuss his paper, but my occupation was gone. There were several speakers before me, the historians alternating with the sociologists, and the discussion turned entirely on the nature of history, our side contending that, as usually written and understood, it was not a science. I had jotted down some notes on my big brown envelope, and when called, rather than keep silent entirely, I explained the situation, showed them my fine typewritten speech, and then finished my ten minutes by an extempo quasi-humorous speech, characterizing history as not a science but a pleasant pastime, requiring no exercise of the reasoning faculties. It made them laugh, but was well received.

On January 21, 1904, Prof. Frank A. Fetter, Secretary of the American Economic Association, asked me for my discussion of Professor Giddings's paper, and I sent him the one I had written out before going to the meeting and which was not read. A rough stenographic report of the remarks I did make was received later, but I suppressed it. No proof was sent me and several typographical errors occur.

Almost forgetting that I had called this paper by this name, I gave a lecture on the *Rise of the Proletariat* before the Rand School of Social Science in New York on December 30, 1909, and another with the same title at the University of Wisconsin on July 26, 1910. Both of these were delivered from notes, but followed somewhat the same lines. They were, however, wholly different from the present paper.

Publications of the American Economic Association, Third Series, Vol. V, No. 2, pp. 431–434 [187–190]. Papers and Proceedings of the Sixteenth Annual Meeting. Part II, New Orleans, La., December 29–31, 1903. May, 1904. Published for the American Economic Association by the Macmillan Company, New York.

502. Discours d'Ouverture

History.—Written June 21, to July 5, 1903. Delivered July 6, 1903, as president of the International Institute of Sociology, opening the Fifth Congress. I have already mentioned under another head (see supra, No. 476) that at the close of the Fourth Congress in 1900 I was elected president of the Institute for the year in which the next Congress should be held, which was not yet decided, but was later fixed for the year 1903. I arrived in Paris on June 17, but suffering from an attack of *grippe* which hung on till I got back to America. There was considerable business in connection with the Congress, and I had to prepare my *Discours d'Ouverture*. I also wrote a set paper (see infra, No. 504). I jotted down what I had to say about Descartes on June 21, and wove it into the *Discours* on June 24. In this case I did not write any English draft, and it was my first attempt to write French directly. I submitted the rough pencil sketch to Dr. Worms on June 25. His father, M. Emile Worms, read it and made a few suggestions. I then submitted it to a French lady, Mlle. Vallette, who does such work and let her correct the idioms. She returned it on June 29, and I copied it in a plain hand to read. I continued revising it and adding to it till the last minute. I read it when the time came as well as I could and I think it produced a good effect. The tenth volume of the *Annales*, containing the proceedings of that meeting did not come out for about a year. I received my copy on July 16, 1904.

Annales de l'Institut International de Sociologie, Tome X, Contenant les Travaux du Cinquième Congrès tenu à Paris en Juillet 1903, Paris, 1904, pp. 49–55.

July, 1904—Ætat. 63.

503. [L'Organisme Social]

History.—Remarks on the papers of Messrs. Casimir de Kelles-Krauz, Ferdinand Tœ~~m~~nnies, and John S. Mackenzie on the relations of Sociology to Psychology, read July 8, 1903. In these papers the subject of collectivism and individualism was discussed, and Kovalewsky insisted on my speaking. I made two ten-minute speeches setting forth my doctrine as I have stated in my article on *Herbert Spencer's Sociology* (supra, No. 498), which had not been written at that date, but I had more briefly treated the subject in *Pure Sociology* (p. 567). I wrote out these remarks on July 19, submitted them for literary correction to Mlle. Vallette, copied them on July 21, and took them to Dr. Worms on July 22.

Ibid., pp. 302–304.

July, 1904—Ætat. 63.

504. Les Rapports entre la Sociologie et la Psychologie

History.—Written June 25, to July 1, 1903. The subject of this Congress was the relations of sociology to psychology and I had something to say on it. So, although president, I decided to have a paper on that subject. I wrote it at short sessions on account of being sick and having the opening address to write. I finished the rough pencil sketch on June 27, and submitted it to

Mlle. Vallette, who returned it with the *Discours d'Ouverture* on the 29th. I then made a neat legible copy to read. It came up at the afternoon session of July 8. I called vice-president Roberty to the chair and read it. At the end of the written paper I made some off-hand remarks on the question of secularization of education in France, which was then a burning question. I told them that we Americans were anxiously watching the experiment and wished it every measure of success. After the meeting a Catholic priest who was somewhat prominent in the meeting interviewed me and questioned me narrowly on the subject. He was, however, quite liberal-minded. I have sometimes thought that this little speech might have had something to do with the conferring on me of the degree of Officer of Public Instruction (see supra, No. 495). I wrote out this remark and I find it at the end of the rough pencil draft, but when I copied the paper for publication I deemed it prudent to omit it. It is as follows:

> "Or l'art qui correspond à cette science est l'instruction publique, et la France a aujourd'hui devant elle précisément ce problème de remplacer dans les endroits les plus reculés et dans les couches les plus basses et arrièrées les idées mythologiques, fausses, et dangereuses [fabuleuses, qui menacent la paix, l'état, et la patrie] par des faits scientifiques, des connaissances pratiques et utiles, et les idées vraies, qui vont rendre la France solide et son peuple heureux. Les yeux de toutes les nations sont fixés sur cette grande expérience sociologique, et tous les bons esprits espèrent que la France aura la sagesse de la rendre un succès complet [qu' aucune intimidation, et aucune sympathie fausse n'empêcheront la France d'atteindre son but]."

The words in brackets occur in my manuscript, but are crossed out and the others substituted. They are probably more exactly what I actually said, and the substitutes are in the nature of an improvement.

I find among the manuscripts an English translation of this paper, carefully made, probably at a later date with a view to possible publication, but this has never been done.

Ibid., pp. 337–340.

505. Discours de Clôture

History.—Delivered extemporaneously on July 9, 1903, at the close of the Congress, and written out for the General Secretary July 19. With all the work I had had to do in Paris, as indicated by the last three heads, and in my state of health, I had not been able to prepare any closing address, and I had intended simply to declare the Congress closed without a speech. But there were things I wanted to say, and during the last session I jotted a number of them down on a slip of paper. I was unusually ill that afternoon and the proceedings dragged in an aggravating way, so that when at last, near six o'clock, when all was done and I must do my final task, I was completely exhausted. But I rallied to the occasion and made this speech, occupying about twenty minutes. My comment on it as written in my diary the same evening is as follows:

"It was nearly 6 o'clock when the time at last arrived for me to make my 'Discours de clôture.' I had had no time to prepare it, and all I had done was to jot down on a piece of paper on the table before me during the afternoon a few heads in English as a reminder of what I ought to say. I spoke about 20 minutes and gave a broad sketch characterizing that remarkable Congress, the most interesting thus far. I also thanked the spectators for the inspiration of their presence. Then I pronounced the Congress closed. I spoke with great deliberation, summoning every particle of strength I possessed, and I believe upon the whole it was a rather neat speech. Worms, Delbet, and others congratulated me on it. Thus ended the 5th Congress of the Inst. Int. Soc., in presiding over which I received the greatest honors of my life, but my state of health rendered it a very laborious task."

Ibid., pp. 415-417.

July 1, 1904—Ætat. 63.

506. The Pteridospermaphyta

History.—Written May 19–23, 1904. I wrote it in pencil on May 19, and it was typewritten on May 23. I sent it to *Science* on the 24th. No one seems to have adopted my term, but it must have attracted some attention, as Mr. Robert Kidston wrote me to send him a copy which I did. It was preliminary to a more extended article on *Paleozoic Seed Plants* (see infra, No. 508).

Science, New Series, New York, Vol. XX, No. 496, July 1, 1904, pp. 25–26.

July 10, 1904—Ætat. 63.

507. A Famous Fossil Cycad

History.—Written in its present form March 22, to April 5, 1904. This consisted in the rewriting of the paper written in rough form October 26, to November 14, 1893, on *The Dresden Cycad*, which was read on the last named date before the Biological Society of Washington (see supra, No. 492). The specimen was described in the Dresden Museum on August 27, 1903 and the species was described on October 31. The letters to Deichmüller and Tietze were sent them on November 13, and answers received before the end of the year. In the light of these letters and further investigations made, I revised the original paper March 18–21, 1904, and then rewrote it and prepared the illustration. It was sent to Prof. E. S. Dana, Editor of the *American Journal of Science*, on April 5, and accepted by him on the 9th. On account of its length there was some delay, and proof was not received till June 14. A copy of the number containing it was received on July 11, and reprints on the 13th.

This paper records one of the boldest pieces of scientific work that I have ever done, and one of the comments that was made upon it was that of some surprise that a famous specimen in a celebrated museum of Germany should have to wait a century and a half for an American to go over there and show them which end up it ought to stand.

I made an egregious mistake in the third line of the paper in saying "more than two centuries and a half," when the dates given show that I ought to have said "more than a century and a half." Probably few ever noticed it. When pointed out to me all I could say was that I might as well make a mistake of a century as of a year.

The American Journal of Science, Fourth Series, New Haven, Vol. XVIII [whole number CLXVIII], July, 1904, pp. 40–52.

August 26, 1904—Ætat. 63.

508. Paleozoic Seed Plants

History.—Written August 6–13, 1904. Prof. John M. Coulter having replied to my short note on the *Pteridospermaphyta* (see supra, No. 506), in such a manner as to show that he, in common with most botanists proper, was almost entirely ignorant of the subject, I was prompted to write this article simply to inform him and the rest what had been done in this line. I still believe that the classification I proposed is the most logical, convenient, and perfect that could be adopted. I made a careful, pencil sketch of the article, which required much research, and then had it typewritten. It was finished and sent me on August 13 and proof corrected on the 18th.

Science, New Series, New York, Vol. XX, No. 504, August 26, 1904, pp. 279–281.

509. [Death of Gabriel Tarde]

History.—On June 3, 1904, I received a letter from Prof. Alfred Espinas announcing the death of Gabriel Tarde. His books were famous and I had read them, but the part he took in the Congress of the International Institute of Sociology of 1903, over which I presided, and the making of his personal acquaintance on that occasion, brought his death very closely home to me, and I felt an inspiration to express my sense of his loss to our science. I therefore sat down on June 5, and penned a strong letter to Professor Espinas and another to Dr. Worms. Of course I had no thought of the publication of either. The regular formal family announcement of his death reached me on June 18. It is on the order of all such announcements customary in the Old World, but for which Americans have no taste. When on November 27, I received the *Revue Internationale de Sociologie* for August and September I discovered that Dr. Worms had translated and published there the part of my letter relating to M. Tarde. As it is something that I wrote and that was published, it belongs here. I am, however, very sure that in speaking of the part taken by Tarde at the Congress I said that of 1903, and not that of 1900, as here printed, and I make that correction as simply of a typographical error. I did not meet Tarde at the Congress of 1900, and do not think he was present. Tome X of the Annales contains a paper of his on Cournot, but I think he simply sent it in for publication. I am sure he did not read it.

Perhaps it is proper to remark at this place that the Section of Social Psychology of the Congress of Arts and Science of the Universal Louisiana Purchase Exposition of 1904 in St. Louis arranged for some remarks on Gabriel Tarde on September 23, and I was one of the speakers. Dr. Edward A. Ross also spoke. These speeches were not published. I spoke ten minutes and gave a description of the man from my knowledge of him, especially his manner of speaking before the Congress, his intense interest in his subject, his peculiar gestures, in which he actually seemed to talk with his fingers. I also mentioned his kindly manner toward all, and the gentle way in which he showed me to the proper tramway on the evening after our reception at the home of M. Espinas on July 11, 1903, the last time I should ever see him.

Revue Internationale de Sociologie, Paris, XII^e Année, No. 8–9, Août-Septembre 1904, p. 654.

WASHINGTON, 5 juin 1904.

CHER ET HONORÉ COLLÈGUE,

Je viens de recevoir du professeur Espinas la triste nouvelle du décès de M. Tarde. Depuis la mort du regretté Coste, je n'avais point reçu de coup pareil. La France, la science et le monde ont fait une irréparable perte. La disparition d'aucun autre Français, peut-être, ne nous toucherait si intimement, nous Américains. Les œuvres d'aucun autre n'étaient aussi connues parmi nous. Son génie propre semblait en harmonie avec la pensée américaine sur tous les grands sujets qu'il avait si remarquablement débattus.

Je le connaissais par ses œuvres avant de le rencontrer, mais nos relations personnelles ne firent qu'accroître mon admiration pour lui. Ses brillantes discussions à notre Congrès de 1903 me montrèrent qu'il était aussi exceptionnel comme orateur que comme écrivain. Elles me firent voir aussi tout ce qu'il y avait de loyal et de simple dans son caractère, son complet oubli de lui-même et son dévouement à la vérité.

Certain en parlant ainsi d'exprimer la pensée et l'émotion de tous ceux qui le connurent, je n'ai pu résister à l'impulsion qui me portait à le faire, et je ne connais personne qui réponde aussi pleinement que vous-même aux sentiments ici exprimés.

Votre dévoué collègue,

LESTER F. WARD.

December 16, 1904—Ætat. 63.

510. [Monument to Bernard Renault]

History.—I received a circular about December 1, 1904, announcing the proposed monument and asking for subscriptions. I could not do much in the financial way, but I thought I might help the matter by putting this note in *Science*, so I wrote it and sent it to Dr. Cattell. He kept me supplied with postal cards adapted to this purpose and I wrote it on one of them.

When I visited Renault in 1900 (see supra, No. 475) he seemed in perfect health. I called on him on July 21, 1903, and was shocked at his condition. He had nearly lost his eyesight and looked completely broken down. He was normally a large well-proportioned man, but now he seemed like a much smaller man, thin and emaciated. I had a short interview, mostly about his health, and I advised him to stop work. But it was too late. He had less than three more months to live.

Science, New Series, New York, Vol. XX, No. 520, December 16, 1904, p. 855.

A SUBSCRIPTION, headed by M. Albert Gaudry, has been started for a monument to M. Bernard Renault, well known for his valuable researches into the micro-organisms of the coal measures, and especially of the bogheads and of the cannel coal of the United States, who died from overwork and in limited circumstances on October 14, last. The monument will be erected at his native place, Autun, where he is buried, and subscriptions should be sent to M. Berthier, secrétaire de la Société d'histoire naturelle, 2, rue de l'Arbalète, Autun, France.

December 30, 1904—Ætat 63.

511. Review of The Jurassic Flora. By A. C. Seward, F.R.S. Part I, The Yorkshire Coast. London, 1900. Part II, Liassic and Oolitic Floras of England (Excluding the Inferior Oolite Plants of the Yorkshire Coast), London, 1904. Catalogue of the Mesozoic Plants in the Department of Geology, British Museum (Natural History). Parts III, IV

History.—Written November 19 to December 15, 1904. Part I had long been in hand and in use, and Dr. Cattell's letter of November 11, requesting me to review Seward's Catalogue of Mesozoic plants referred to Part II only. As I had both already I could begin at once, but I had very little time to devote to it. Many days it was not touched, other days I would give it an hour, a half hour, or only 15 minutes. But I wrote it out carefully in a rough pencil draft, which was finished on December 14, and the typist copied it the next day. Proof was received on December 22. (For the review of Seward's Wealden Flora see supra, Vol. VI, No. 456.)

Ibid., No. 522, December 30, 1904, pp. 917–920.

January (?), 1904—Ætat. 63.

512. Administrative Report for the Year Ending June 30, 1904

History.—Compiled in the usual way from the monthly reports and embodied in condensed form in the report of the Director. This report covers the last work I did for the Geological Survey. It will be seen that I had actually commenced my third paper on the *Status of the Mesozoic Floras of the United States*, but no work was done on it after June 30.

Twenty-fifth Annual Report of the United States Geological Survey for the fiscal year ending June 30, 1904, Washington, 1904, pp. 62–63.

January, 1905—Ætat. 63.

513. Le Sujet-matière de la Sociologie

History.—This, as stated in a footnote, is Chapter III of the French translation of *Pure Sociology*. Its insertion here would be an unnecessary duplication.

Revue Internationale de Sociologie, Paris XIIIe Année, No. 1, Janvier 1905, pp. 1–28.

January 20, 1905—Ætat. 63.

514. An Example in Nomenclature

History.—Written December 27–30, 1904. Mr. White's paper appeared on December 23.

Science, New Series, New York, Vol. XXI, No. 528, January 20, 1905, pp. 110–111.

March 30, 1905; October 3, 1906—Ætat. 63, 64.

515. Evolution of Social Structures[1]

History.—Written August 26, to September 10, 1904. On March 23, 1903, Dr. Small wrote me that he would like to have me present one of the two leading papers at the Congress of Arts and Science of the St. Louis Universal Exposition. These papers were to be on the *Method of Sociology* and the *History of Sociology* respectively. He said that Professor Giddings would give the other. And he added:

> "I hope that you and Giddings can agree about division of territory. I have written him to this effect. My own preference would be that you should treat *Method*, and he *History*, for the reason that you have just given us a magnificent outline of the history and he has expressed himself so extensively on method."

I answered his letter on September 26, and I think I said that I could treat the *Method*, but that I was not situated so that I could properly treat the *History*, as the literature was not sufficiently within my reach.

I heard nothing further of the matter till December 22, 1903, when I received from him my official invitation dated December 19, to present a paper in Department 16, Social Structure, on *Progress in Sociology*. I do not think I replied to it, but I saw him at New Orleans a few days later and on December 30, we lunched together and talked over many things including this subject. I am pretty sure I repeated what I had said about not being able to treat the history of sociology, but it did not occur to me that this was what was implied in the word "Progress." On January 20, 1904, I received the official letter of the President of the Congress and Chairman of the Organizing Committee, Professor Simon Newcomb, inviting me

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[1] A paper read at the International Congress of Arts and Science, Department of Sociology, St. Louis, September, 1904.

> "to become a member of the Congress in the Department of Sociology (Department 16), and to deliver an address on one of the phases of that subject mentioned on page 9 of the programme, namely, *Progress during the Last Century*, or *Fundamental Conceptions and Methods*."

In the Programme the Department of Sociology (16) occurs on page 24, and the words in the letter occur on page 5, not on page 9. It was all ambiguous and I made no reply and took no steps in the matter. On March 3, Professor Newcomb called on me to ask whether I would deliver the address. He gave me my choice of subjects and I chose the *Method*. On April 4, I received a letter from Professor Newcomb saying that Dr. Small had learned that Professor Giddings had already commenced his address on the *Method of Sociology* and asking me if I was willing to give the one on the *History and Progress*. I replied immediately that I could not, and considered the whole matter settled. And so it was, so far as the two principal addresses were concerned, and the one assigned to me was given by Professor George E. Vincent.

On August 10, 1904, Dr. Small wrote me that Dr. Tönnies, who was to give one of the addresses on *Social Structure* in Section (b), was prevented from doing so for family reasons, and he asked me if I could take his place. In the same letter he asked me to speak on *Gabriel Tarde* in the Section of Social Psychology (see supra, No. 509). I had been long at work on social structure and I saw that I could soon write a paper on that subject. I had some desire to go to St. Louis, but had given it up. I deliberated a good while, and finally, on August 22, I wrote agreeing to present a paper. He had said I could say anything I pleased. There was not much time to lose and I got at it very soon. I did not make any rough draft, but put it right through in final form. I have just looked through the manuscript, which was returned with the proof, and I find that there is not a single interlineation and only one word was struck out in the revision. It is exactly like a final draft. It was really finished on September 2, but I had called it simply *Social Structures*, and on the 10th I changed the title to the one finally adopted, requiring the rewriting of the first folio. On September 16, I got a letter from Dr. Small saying that Dr. Tönnies had changed his mind and was coming, but that he wanted my address, too.

The two leading addresses (by Vincent and Giddings) were delivered on September 20, but I did not reach the park till the hour (4–30 P.M.) that they were to come off, my train being late, and I had to locate myself and could not hear them. But the Social Structure Section held its meeting on September 21, and all the addresses (Ratzenhofer's, Tönnies's, and mine) were read in the forenoon. Ratzenhofer read his in German, but Dr. Small's printed translation was distributed to members. Tönnies read his in English, but so broken that I could not follow him as easily as I could Ratzenhofer. I made the acquaintance of both, and Ratzenhofer spoke French with me. They both went to Washington later and Ratzenhofer called on me at my house where we had the only conversation of any length that I had with him, his health being so poor that we could not arrange an interview in St. Louis. As all know, he died on the voyage back to Europe. Tönnies I met again at the Cosmos Club in Washington, and we were both guests of Lady Welby during the Congress of the International Institute of Sociology in London in July, 1906 (see infra, Nos. 533, 534). I handed my paper to Dr. Small as soon as read and he published all the papers of the Department of Sociology somewhat in the order in which they stood on the programme. Proof of mine was received on March 4, 1905, and the number containing it reached me on April 1. My reprints came April 15.

All the papers read before the Congress of Arts and Science were published in the proceedings of the Congress, which form eight large volumes, but the work of editing and issuing them was slow. On January 5, 1905, I received a blank to fill out giving my biography to stand at the head of my paper when it should appear in the proceedings. I filled it out and returned it the next day to Mr. Howard J. Rogers, Director of the Congress, and it was so treated. On February 6, Mr. Rogers sent me a circular requesting me to supply a bibliography, or as it reads, "a few notes concerning the most important literature of the subject." I did not do this at once, and on March 4, as above stated, I received the proof of my paper from Chicago, and it occurred to me that the bibliography might appear in both places; so I prepared it and sent it to Dr. Small with the proof and a letter explaining it. On March 7, he returned the bibliography saying that the others had not supplied it.

SPECIAL WORKS OF REFERENCE FOR THE SECTION OF SOCIAL SCIENCE

(Prepared by the courtesy of Professor Lester F. Ward)

Gumplowicz, Ludwig. *Raçe und Staat, Eine Untersuchung über das Gesetz der Staatenbildung*, Wien, 1875.
Philosophisches Staatsrecht, Wien, 1876.
Der Rassenkampf, Innsbruck, 1883.
Die Sociologische Staatsidee, Graz, 1892, Zweite vermehrte Auflage, Innsbruck, 1902.
Allgemeines Staatsrecht, Innsbruck, 1897.
Geschichte der Staatstheorien, Innsbruck, 1905.
Ratzenhofer, Gustav. *Wesen und Zweck der Politik, als Theil der Sociologie und Grundlage der Staatswissenschaften*, 3 vols. Leipzig, 1893.
Die Sociologische Erkenntnis, Positive Philosophie des Socialen Lebens, Leipzig, 1898.
Simons, Sarah E., *Social Assimilation*, American Journal of Sociology, vol. vi, May, 1901, pp. 790–822; vol. vii, July, 1901, pp. 53–79; September, 1901, pp. 234–248; November, 1901, pp. 386–494; January, 1902, pp. 539–556.
Vaccaro, Michelangelo. *Les Bases Sociologiques du Droit et de l'Etat*, Paris, 1897 (translated from Italian: Le Basi del Diritto e dello Stato). Roma, 1893.

After receiving my reprints I thought they might serve as manuscript for the republication in the proceedings, and therefore I sent a copy to Mr. Rogers on April 20, and accompanied it with the bibliography. He acknowledged it on April 21, and at the same time offered to send me a few reprints. As I supposed these would be sent I did not specially order any.

On November 15, 1905, I received the Commemorative Diploma of the award conferred upon me by the Superior Jury of Awards, and a letter from President Francis stating that a Commemorative Medal had also been conferred. Circulars explaining the design of both accompanied the Diploma, and one explaining delay in sending the medal, which did not arrive till February 19, 1906. On January 30, I received another letter from President Francis saying that the Proceedings of the Congress (8 volumes) would be sent to me, and I received them as fast as they appeared. The proof of my paper reached me when I was in Paris, May 21, 1906, forwarded to me there from Washington. On October 4, Vol. V, containing my paper was received. I wrote to the publishers for the reprints that had been promised, and they replied that the type of that volume had been distributed. The bibliography was not put at the end of the article, but all the bibliographies were put together at the end of the volume, a very awkward proceeding. This paper was reprinted entire in the *Plebs Magazine* for February and March, 1911.

The American Journal of Sociology, Chicago, Vol. X, No. 5, March, 1905, pp. 589–605; Congress of Arts and Science Universal Exposition, St. Louis, 1904, Vol. V, Boston and New York, Houghton, Mifflin and Company, 1906, pp. 842–855; Bibliography, p. 888; The Plebs Magazine, Oxford, Vol. III, No. 1, February, 1911, pp. 9–14; No. 2, March, 1911, pp. 33–40.

April 10, 1905—Ætat. 63.

516. Menthaceae

History.—On April 23, 1903, soon after I commenced work on the *Century Dictionary* the second time, Messrs. F. V. Coville, and Charles L. Pollard joined me in a council over the names of families of plants, which must now, under the new rules of nomenclature, all be based on the name of some prominent genus. We decided upon about all of them and several had never before been published. Their appearance in the *Supplement* to the *Century Dictionary* would therefore be their first publication. The mint family was in a very bad state. Labiatae must go, and the question was which of the other names that had been given to it to adopt. Strange to say no one had thought of naming it after the mint genus, *Mentha*. I proposed that we reject all the old names and adopt the name *Menthaceae*. This was unanimously agreed to, and this name has since been used in all government publications. In the spring of 1905, Mr. W. E. Safford was writing his report on the flora of Guam and describing the plants collected there by him, which of course included some belonging to this family. He was aware of the decision to adopt my name, and when he reached the mint family he introduced it in its proper place. But as it had never been published before he considered it proper to describe it briefly, and as I had named it he and Mr. Coville, head of the botanical division, joined in asking me to characterize it. I then wrote the paragraph of his report placed in quotation marks and credited to me. Under the new rules of nomenclature the law of priority, while proper to be followed in all groups, other things equal, does not apply to families that have not received suitable names.

In fixing on these new names in 1903, we supposed that they would be first published in the *Supplement* to the *Century Dic-*

tionary, but that did not appear till January, 1910, and most of the names had then been published in other works. When I reached this one in its alphabetical order I defined it, and my definition stands in its proper place in the columns of that work. It is as follows:

Menthaceæ (men-thā′cẹ̄-ē), *n. pl.* [NL. (Ward, 1905), <*Mentha* + *-aceæ.*] A great family of dicotyledonous sympetalous plants of the order *Polemoniales*, the mint family, typified by the genus *Mentha*, the mint genus. It is characterized primarily by a labiate corolla, whence it was called *Labiatæ* by Bernard Jussieu in laying out the garden of the Trianon—a name which has been generally adopted and is still widely used, although not based on that of any genus of the family. See *Labiatæ* [in the Dictionary proper].

Smithsonian Institution. United States National Museum. Contributions from the United States National Herbarium. Volume IX.—The Useful Plants of the Island of Guam with an Introductory Account of the Physical Features and Natural History of the Island, of the Character and History of its People, and of their Agriculture. By William Edwin Safford. Washington. Government Printing Office, 1905, p. 324; The Century Dictionary Supplement. Prepared under the Superintendence of Benjamin E. Smith, A.M., L.H.D., Managing Editor of the Century Dictionary and Editor of the Century Cyclopedia of Names and the Century Atlas. The Century Dictionary and Cyclopedia. A Work of Universal Reference in all Departments of Knowledge with a New Atlas of the World. Vol. XII. Published by the Century Co. New York [1910], p. 787.

The author of the name *Menthaceae*, which is here published for the first time, is Prof. Lester F. Ward, who has presented the following statement regarding it:

"Permit me to propose the name Menthaceae for the mint family as the most suitable substitute for the Labiatae, given it by Bernard Jussieu in Hort. Trianon, 1759, and used by most botanists since that date, but which has not the proper termination and is not formed from the name of any genus of the family. Being based on Mentha, the most typical genus of the family, it has better claims, except in the matter of priority, than Lamiaceae (Lindley, 1836), Nepetaceae (Horaninow, 1843), or Salviaceae (Drude, 1879)."

May 22, 1905—Ætat. 63.

517. Review of an Outline of the Theory of Organic Evolution, with a description of some of the Phenomena which it explains. By Maynard M. Metcalf. New York: The Macmillan Company. 1904. 8°, xxii+204 pages, illustrated

History.—Written January 10–11, 1905. The book was sent me November 30, 1904, and read December 24–31. Proof was received April 10, 1905, and the number containing the review, as also a supply of extra leaves out of it, on May 22.

The American Anthropologist, New Series, Vol. VII, No. 1, Lancaster, Pennsylvania, January–March, 1905, pp. 117–118.

518. A Text-Book of Sociology. By James Quayle Dealey, Ph.D., Professor of Social and Political Science in Brown University, and Lester F. Ward, LL.D. of the Smithsonian Institution at Washington, D. C. New York. The Macmillan Company, 1905

History.—On May 13, 1903, I received the following letter from Prof. J. Q. Dealey, who was then a stranger to me:

BROWN UNIVERSITY, PROVIDENCE,
May 12, 1903.

DR. LESTER F. WARD,
Washington.

DEAR SIR:

I have been a Prof. of Social and Political Science at Brown for eight years and have at various times handled in Senior and Graduate classes your *Outlines* and *Psychic Factors*. This term, in a small class of 14, I am using your *Pure Sociology*. We are delighted with the book and the young men have expressed a strong desire to see and hear the author. If you happen this way, I hope you will take occasion to let us know.

Prof. Wilson (Geo. G.) and I have been anxious for years to find a book suited for our elementary class in sociology. We have tried nearly all and like none. It has occurred to me that an abridged edition of *Pure Sociology* would be just the thing. The full edition is too large and too expensive for so large a class (125-175 persons each year), but an abridged edition of about half the size and price would prove satisfactory. You may plan to issue such an edition. If not, I'd be glad to have you consider the feasibility of entrusting me with the task. I should be glad to undertake it because of the high character of the book, and also because I would thereby be able the better to adapt it to the use of what must be the largest class in sociology in the country. I shall be glad to hear from you in regard to this matter and the visit. Prof. Wilson heartily joins me in the suggestion.

Yours truly,

J. Q. DEALEY.

On the 17th of May, I replied saying that I was quite willing that such a book should be prepared, that not being a professional teacher myself, I would not know how to condense and adapt my works, and that I would gladly entrust the matter to him. He immediately laid the matter before the Macmillan Company and they replied that they did not deem it wise to issue such an edition at once, not for a year at least, lest the new book should interfere with the sale of the *Pure Sociology*.

Professor Dealey waited the year and then renewed the effort by writing again to the Macmillan Company. They referred his letter to me in one dated June 21, 1904. They asked me first if I could prepare the text-book myself, and if not whether I would be willing to let it be done by another. As soon as I received it I wrote to Dr. Edward A. Ross, then Professor of Sociology in the University of Nebraska, one of whose books (*Social Control*) that Company had published in 1901, laying the whole matter before him, and enclosing the correspondence thus far. I told him I would like to have a letter from him to show to the Macmillan Company. Dr. Ross sent me a strong letter which I sent to the Company returning Professor Dealey's, and on July 5, they replied, returning Dr. Ross's letter, and inquired again whether I wished to condense the book myself. I replied that I did not and was satisfied to have Professor Dealey do it. The matter lay fallow the rest of the summer, but when I was in Providence to give a lecture to the Women's Club we revived it and canvassed the whole subject. Hitherto we had talked only of a condensation of *Pure Sociology*, but now there seemed to be a desire to make the text-book embrace matter in other works of mine, especially *Dynamic Sociology*. Soon after that Professors Wilson and Dealey renewed their efforts with the Macmillan Company, and the latter accepted their propositions. The correspondence was laid before me on November 14. By the 25 it was fully arranged and the table of contents drawn up. The agreement was signed by all parties on December 2. It provides that the royalty shall be equally divided between Professor Dealey and myself.

On January 2, 1905, I received from Professor Dealey practically the whole manuscript of the book. He had cut up two copies of *Pure Sociology* and selected and pasted on sheets the passages he wanted to use. He had also used matter from the

Outlines of Sociology. I looked it all through carefully and returned it to him on January 6. On the 17th he asked me to send him proofs, which I had told him I had, of *Dynamic Sociology*. I have preserved galley and page proofs of all my books. He wanted Chapters XIII and XIV and I sent them to him. The preface, though signed by us both, was written by Professor Dealey, who submitted it to me on February 18. At that date he stated that he had already sent the manuscript to New York. On March 31, proof began to arrive, and the book was all in type by May 8, and in pages on the 18th. Professor Dealey then prepared the index, and that was finished on June 12. On July 8, I received copies of the book.

On May 24, 1909, I received a letter from Sr. Antonio Ferrer y Robert in Barcelona, Spain, asking my permission to translate the *Text-Book* into Spanish. I knew that Professor Posada and his assistants were at work on my books, and this one had been mentioned in one of his letters. I therefore wrote to Posada asking if Ferrer y Robert belonged to his collaborators. I told him in that letter that I was going to Europe in June and would be in Berne on July 20, adding that I hoped I might meet him there at the Congress of the International Institute of Sociology. It was exactly at that time and place that I did meet him for the first time, and he then informed me that he had communicated with Ferrer y Robert, and told him to go ahead with the translation. I received a letter from the latter on September 2, saying that he had heard from Posada. I wrote encouraging him to go on, which he did and wrote me several letters, asking for photographs of Professor Dealey and myself, also asking for a short foreword from us to put into the book, all of which we sent. On February 12, 1910, I received a letter from him saying that the translation was finished, and that he had handed it to his publisher on January 28. He said he thought it would appear before the month of June. He also stated in that letter that he had asked Professor Posada to write a prologue for the book, and for permission to dedicate it to him.

There were prolonged delays, but Ferrer y Robert wrote several times reporting progress and promising soon to send us copies of his translation. Such a letter bears date December 1, 1911, and reached me during that month, but no copies arrived, and I was thinking of writing to know if they had been sent and perhaps miscarried, when at last, on February 3, 1912, I received two copies by registered mail. The work is dedicated to Posada, but he contributed nothing to it. The prologue drawn up by Professor Dealey on July 14, 1910, and signed by us both, follows the dedication. The title-page is as follows: Manual | de | Sociología | por | James Quayle Dealey, Ph.D. | Professor de Ciencia Social y Politíca | en la Brown University | y | Lester Frank Ward, LL.D.| de la | Smithsonian Institution de Washington, D. C.| Traduccion de Inglés, con Autorización de los autores | de | Antonio Ferrer y Robert| Abogado del ilustre Colegio de Barcelona | ⊞ | F. Grana day C.ª Editores | 344—Diputación—344 | Barcelona.

October 13, 1905—Ætat. 64.

519. Status of the Mesozoic Floras of the United States. Second Paper

History.—Written July 11, 1901, to June 3, 1903. Revised and condensed November 5, to December 17, 1903. Further condensed April 16, to May 27, 1904. The first paper of this series (see supra, No. 471) went to press on January 24, 1900, and appeared on August 13, 1900. Nearly a year was spent in preparation to begin the second. Large collections had to be determined and much field work done, but the entire summer of 1900 (May 24 to October 22) was spent abroad. On July 11, 1901, I was ready to begin the actual dictation of the second paper. I began with the results of my field work in Arizona in May and June of that year. This was completed on August 19, and I then commenced work on the Jurassic Flora of Oregon. Professor Fontaine had described the plants and his report was in my hands. It had to be edited, classified, and made to conform to modern terminology, as in all other cases. The types had to be drawn in the Division of Illustrations, and I had to attend to that. This chapter was about completed by the middle of September, and on September 22, I commenced on the Jurassic Cycads from Wyoming. The specimens had to be photographed and ten days were spent in getting this done. I began work on the Cretaceous flora on September 28, taking up the Shasta formation, from which I made a large collection in 1879. This had also been determined by Professor Fontaine and I must edit his report.

About this time there had been complaints about the great cost of illustrations in the various reports of the Geological Survey, and the Appropriation Committee had cut down the appropriation for 1901 fifty per cent. from previous years. The Director of the Survey appointed a committee to examine the matter. They found that paleontology was the source of the heaviest expense, and decided to limit it. The illustrations in my first paper, especially those of fossil cycads, were criticized as excessive, and they sent for me to explain how they could be curtailed. I met the committee on October 19, 1901, and promised to retrench as much as possible in this paper. I had to go all through the types of Shasta plants and reject many that Professor Fontaine had designated to be drawn. This laborious work had to be done for all his reports.

I had intended to publish this paper in the Annual Report the same as the first one, but the tendency to swell the Annual Reports had become so obnoxious that I feared I would have to make a Monograph of this. The Committee intimated as much to me. In that case I hoped I need not have to sacrifice so many figures.

On November 12, 1901, I had finished editing the Shasta report. At the same time with this work I was preparing the Jurassic cycads from Wyoming. There were hundreds of small specimens and they were covered with lichens, making their determination impossible. I had to put them in a large earthen tray filled with sulphuric acid, which loosened the lichens and enabled me to remove them. Only a few could be thus soaked at a time and every day for months I had to change them and clean them, which took about two hours a day. As the acid ruined all it touched and burned the fingers, I had to wear an apron and handle the specimens with instruments. This work was finished November 29.

On November 18, I commenced work on the Kootanie flora. My note-book shows that I was working up the Jurassic cycads simultaneously with the Kootanie flora. Wieland's notes were received on November 23, and incorporated in my rough manuscript. On December 13, I was ready to commence editing Fontaine's report on the Kootanie flora. This was finished on the 19th. I finished determining the Jurassic cycads on December 31. Their systematic description was begun on January 3, 1902. This work took till January 11. I spent January 13–17 in Baltimore at the Woman's College describing the new cycad trunks that Mr. Bibbins had found. These descriptions entered into my chapter on the Maryland Cycads. The Cape Lisborn flora was written up January 21–22. On the 24th I commenced writing up the Dakota formation (Black Hills). This was done on February 1. On February 3, I commenced on the Trinity formation and finished it on the 17th. On the 18th I began the much larger task of writing up the Older Potomac flora. It took me till March 5, to go over the literature and get out the correspondence. Then I had to stop and work in the Jurassic plants from Alaska. This report and several other small ones had to be interpolated in my manuscript in their proper stratigraphic places. I did not commence writing up the Older Potomac till March 26. There were many Potomac localities that I wanted to revisit before I finished that chapter, and I commenced making field excursions for that purpose, beginning on March 25. I went on fair days, always returning the same day. I kept this up the whole spring and summer of 1902, not taking the field at any distance from Washington. There were certain points that I wanted to consult Professor Fontaine personally about, and on April 21, I went to Charlottesville and spent a day with him at the University of Virginia. We opened several of the boxes he had and I found what I wanted. We discussed the whole subject of the Potomac formation. I also had to make a second visit to the Woman's College, Baltimore, on April 28, to go over locality notes and maps with Mr. Bibbins. On May 1, I finished the historical matter down to the season of 1902, and had to stop and wait for Professor Fontaine's and Mr. Bibbins's reports. That historical review is a thorough and exhaustive piece of work, and occupies over 60 pages of the Monograph.

On May 19, I learned that the bill had passed providing for "Professional Papers" by members of the Survey staff that should not be subject to the "iron-clad" provision of the Monographs (see supra, No. 471) and I called on the Chief Geologist and members of the committee on retrenchment. They decided then and there that my paper should be published in that series. For reasons that I never learned this promise was not kept.

A large amount of time was now spent on the illustrations. Most of the drawings had been made, and it devolved upon me to arrange them in the order in which they were described in the manuscript and mount them on plates of the proper size. The quarto size had been determined upon and I proceeded to mount them. The Jurassic cycads were photographed June 24–26, and when the views came I mounted them in like manner. This was a prolonged labor and was not completed until well into August. The editing of Bibbins's contribution cost much time and it was unsatisfactory at best.

On Sept. 2 I commenced writing the descriptions of the species of Maryland cycads. This was completed on the 27th. The mounting of the plates illustrating the Potomac cycads was a difficult task. The trouble was with the scale, which was not always shown in the photograph and had to be obtained by actual measurement of the specimens.

The last systematic work was to edit Professor Fontaine's report on the numerous collections that had been made from the Potomac formation since the appearance of his monographic work in 1889. Most of these collections had been made by me, but recently Mr. Bibbins had found a number of important plant-bearing localities in Maryland, and made noteworthy collections. They had all been sent to Professor Fontaine for determination, and were the last to be reported upon. His report was received in November, and the types were sent to the Division of Illustrations to be drawn. All else was completed by November 8, and the manuscript was being engrossed.

On December 19, I commenced editing Fontaine's report on the Potomac plants, which occupied me most of the time till February 5, 1903. It was a great labor. On February 9, I commenced preparing the table of distribution which occupies pages 582–589 of the text. This work was interrupted on February 12, by the arrival of about all the collections that Professor Fontaine had had to determine. The boxes had to be unpacked, the specimens classified by localities and by species, and the types (specimens to be drawn) separated from the rest. This took ten days, and on February 23, I commenced revising the rough manuscript direct from the specimens of the Potomac flora. This work was ended on March 21. I commenced mounting the Potomac plates on March 23, and finished on the 27th. Then I made a final revision of the Potomac chapter, which was done on April 11. Resumed work on the table of distribution on the 13th and finished it on the 18th. Wrote the analysis of it April 20–25. Prepared the columnar section on p. 598, April 29, to May 1. On May 6, I commenced putting the entire manuscript into final shape. There were many minor matters to be attended to. I prepared the list of illustrations May 8–10. Wrote the descriptions of the Potomac plates May 9–13. Nothing remained but to prepare the maps. That of the Shasta region (Pl. LXIV) was made May 14, that of the Buck Mountain region (Pl. V), May 16–22, a map of the Great Falls region where most of the Kootanie plants were collected, which was subsequently suppressed, May 29–30. The map of the Little Colorado Valley (Pl. IV) had long been made, and that of the Maryland Potomac region (Pl. LXXX) accompanied Mr. Bibbins's contribution. A few other details delayed matters till June 3, when the entire manuscript, plates, and maps were sent to the Editorial Department.

On June 6, 1903, I sailed for Europe and did not return till October 13. On November 5 the manuscript of this paper was sent to me with some criticisms made by a committee to which it had been referred and a letter from G. K. Gilbert. They proposed striking out a large part of Bibbins's contribution, to which I had no objection. They were of opinion that the Moencopic beds were Permian and not Triassic. There was no proof either way and I was not certain on that point. On November 7, I discussed all the points with the Chief Geologist and he agreed that Gilbert was wrong in his contention. Some other unimportant matters, mostly redundant remarks in Professor Fontaine's report, were struck out. The result on the whole was satisfactory to me. A certain number of plates had been rejected by the committee on retrenchment, and this involved a great deal of work changing the corresponding parts of the manuscript. This work occupied me until December 17, when the manuscript was returned to the Chief Geologist. On the 24th I received a letter from the chief of the Division of Illustrations saying that the committee had decided to reject a large number of the illustrations. I was discouraged and on January 4, 1904, I recalled both manuscript and plates.

I had no heart to do anything about it, in fact, I was tired of the whole matter, and as I had plenty of other work, I allowed it to lie fallow for three months. On April 9, inquiries were made with regard to my paper. On the 14th I called at the Division of Illustrations and canvassed the whole matter. I tried to save some of the most important illustrations and several of my requests were complied with. I saved all the maps but that of Great Falls (Kootanie), which was the least important. The chief reduction was in the Maryland cycads, where the illustration really was somewhat excessive. As they left it to me to choose which plates to throw out I carefully culled them so as to leave one type at least for each species. This work was slow and laborious on account of the great care needed in making the changes in the manuscript, and on May 3, I returned the accepted plates to the Division of Illustrations. I had reserved all the data and commenced making the changes at once. It was finished on May 23, and I sent the manuscript in on May 27.

Proof of the plates came October 13. It took me till the 29th to finish correcting them. Proofs of the maps reached me on December 9, and proofs of the manuscript on January 31, 1905.

As this was the last work I did for the Geological Survey a word of explanation seems called for just at this point. It had been my ambition to complete the series which I called the *Status of the Mesozoic Floras of the United States* (see supra, No. 471), and I had great collections and most of the data of every kind for doing so. Still considerable more field work would have been required, and several years of hard labor such as I had been doing in the last three or four years. But if all had gone smoothly I should have gone on and completed it no doubt. I had, however, had so much trouble with this paper that, as already said, I was discouraged. The Geological Survey had fallen into the hands of small men, and was no longer the grand institution that it was in the days when Major Powell was its Director. The policy seemed to be to set up captious criticism and obstruct the scientific work of members of the staff. It was a case of bureaucracy. There was no longer any *esprit de corps*, and no one was certain that his work would be approved by petty officers at headquarters. I had been obliged to go abroad several times, and although my work in Europe had always been largely in furtherance of my Survey work, it was not wholly so, and when on the regular roll of the Survey holding a commission and paid by the year, it was difficult to arrange a leave of absence for so long a time. Moreover, ever since 1889, with some intermissions, I had been engaged in supplying the botanical definitions, first of the original *Century Dictionary*, then of the *Supplement* to *Webster's International Dictionary*, and again of the *Revision* and *Supplement* of the *Century Dictionary*, which was not only more profitable financially than any salary the Survey was willing to pay me, but was usually pressing, so that I needed to have more time for it than my unofficial time when keeping office hours for the government. This could be secured by being removed from the regular classified service to a per diem roll, at so much per day when actually employed. This arrangement was made in 1903, before I visited Europe in that year, and from that time I had called on the Survey to reimburse me only for the time that I actually worked for it wherever I might be. It happened that during the whole of 1904, I had a large corps of specialists working under my direction on the *Century Dictionary*, and the matter had become so pressing that the Managing Editor, Dr. Benjamin E. Smith, had asked me in May to give more time to *Dictionary* work. This I had done, and so much time that I had actually done no work for the Geological Survey that I was willing to charge for during the second half of that year. It was going on that way in January 1905, and it seemed to me improper to allow my name to be carried on the rolls when I was not doing any work and not likely to do any for some time to come. I therefore had it taken off and virtually resigned my position of Paleontologist on January 22, 1905.

Proof of this paper began to arrive on January 31, 1905, but I did not have time to correct it, and it was done by Miss Charlotte H. Schmidt, who had been my assistant for so many years, had typewritten the manuscript, and was perfectly competent to correct the proof. I was at my office in the National Museum every day to advise in all needed matters, and saw that it was properly done. The same was true in making the index, which could be done as soon as a complete set of the pages was received, and this occurred on May 10. The index prepared by Miss Schmidt is a creditable piece of work. It was done according to my method, with which she was perfectly familiar. The work appeared on October 13, 1905.

Fifty-eighth Congress, 3d Session, House of Representatives, Document No. 475. Department of the Interior. Monographs of the United States Geological Survey, Vol. XLVIII, Part I,—Text. Part II,—Plates. Washington Government Printing Office. 1905. 4°. Text 616 pages, 11 figures. Plates I–CXIX.

November, 1905—Ætat 64.

520. Les Forces Phylogénétiques

History.—This is an extended extract from Chapter XIV of the French translation of *Pure Sociology*, which, as stated in a footnote, was then about to appear. Vol. II begins with this chapter, and the extract was published under the rule explained when treating of the work (see supra, No. 487; No. 513). The number containing it reached me November 2, 1905. It is excluded from this work according to the same rule that was applied to the extract from the first volume (see supra, No. 513).

Revue Internationale de Sociologie, Paris, XIIIe Année, No. 11, Novembre 1905, pp. 777–824.

January 24, 1906—Ætat. 64.

521. [Announcement of Applied Sociology]

History.—Written September 16, and 17, 1905, at the request of the publishers. They sent me a large supply on January 25, 1906, and I mailed many at once and kept the rest to slip into letters, etc. A large number are still in my hands, because after the book appeared a new one was issued (see infra, No. 524). They were printed on both white and blue paper.

APPLIED SOCIOLOGY

By Lester F. Ward, Professor of Sociology in Brown University

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8vo. Cloth. 384 pages. With charts. List price, $2.50; mailing price, $2.75.

The purpose of this new work on sociology is to point out how the obstacles to human progress may be overcome by the application of the principles of social science and how the human race may be brought into the full fruition of its powers.

It is maintained that the agents of history are men, that human progress is due to the actions of the members of society, or, in other words, that civilization is the product of human achievement. But it is clear that only a comparatively few contribute to this end, and that the result is proportional to the number and efficiency of those so contributing. The problem therefore is how to increase the number and efficiency of the agents of civilization.

It is further shown that the actual number and the actual

quality of these agents are capable of being greatly increased and improved by the removal of unfavorable conditions.

The argument is supported by historical facts, and the work is to a considerable extent statistical. It is illustrated by colored maps, charts, and tabular exhibits.

GINN & COMPANY PUBLISHERS
Boston New York Chicago London
San Francisco Atlanta Dallas Columbus

March 7, 1906—Ætat. 64.

522. The Past and Future of the Sexes

History.—Written January 13–31, 1906. On December 16, 1905, I received a letter from Mr. Hamilton Holt, Managing Editor of the *Independent*, saying that he had just read Chapter XIV of *Pure Sociology* and was greatly interested in the subject. He wondered whether I would be willing to write a popular article for the *Independent* on *The Future of the Sexes.* I was about starting for New York on some business and I hoped to get time to go to the *Independent* office and talk the matter over, but I did not, and it was January 1, 1906, before I replied to the letter. I demurred somewhat on several grounds (very busy with *Century Dictionary* work and reading proof on *Applied Sociology*). I also said that I would not attempt to write an article on *The Future of the Sexes*, as I was no prophet of the future, but that I might call it *The Past and Future*, the future to be inferred so far as possible from the past. After some further correspondence I decided to undertake it. I made a rough draft which I managed to finish on January 30, and copied it immediately and sent it the next day. The art editor asked me for a photograph, which I sent him, and a portrait was made a feature of the article. Proof was received on February 16. On the 24th Mr. Holt wrote me that he had submitted my article to a psychologist, a biologist, and an ethnologist and gotten from them replies to it which he proposed to publish with it, and wanted me to read them and write a rejoinder, the whole to be published in one number of the *Independent* as a sort of symposium. He had given me no intimation of this action, and it seemed to me a breach of professional ethics. He said the psychologist was Dr. G. Stanley Hall, but did not name the others. It was peculiarly objectionable to me because my article was necessarily bald and devoid of facts or evidence in support of a theory opposed to popular ideas. It was scarcely more than a still farther condensation of the brief recapitulation given in *Pure Sociology*, pp. 373–377, of a long chapter in which I had given the evidence. My critics would therefore have me at a great disadvantage and would make short work of me. I therefore wrote him that I did not approve of his plan and that I would not consent to my article appearing in that form, and I told him he might cancel the contract. He replied, pretending great surprise that I should look at the matter in that light, and promising not to publish the other articles in the same number. To this I consented and let the article come out. He afterwards sent me Dr. Stanley Hall's article and part of that of the biologist, who proved to be Prof. Edmund B. Wilson of Columbia. I never saw the ethnologist's article and do not know who he was. As neither of the ones I saw mentioned *Pure Sociology*, or seemed to know that there was such a book, and both based all they had to say on the naked statements of my article, I did not consider them worthy of my attention, and never attempted to answer their arguments. As I expected, they each gave a few cases which seem to be opposed to the theory and imagined that they refuted it. There are plenty of such cases, and I could write a much stronger refutation than these. It is the usual atomistic, myopic method by which all great truths have always been attacked.

On April 7 I received the following letter:

BUREAU OF EDUCATION, WASHINGTON,
April 27, 1906.

LESTER F. WARD, LL.D., 1464 Rhode Island Avenue, Washington, D. C.
MY DEAR PROFESSOR WARD:

I was very sorry to have missed your call on me in office, because I wished to shake hands with you and to congratulate you on your delightful article appearing in the New York *Independent* on the subject of the male and female sexes.

In my opinion you have the victory all to yourself over Dr. G. Stanley Hall on that question.

Very sincerely yours,
W. T. HARRIS,
Commissioner.

The Independent, New York, Vol. LX, No. 2988, March 8, 1906, pp. 541–545.

July 2, 1906—Ætat. 65.

523. Applied Sociology.—A Treatise on the Conscious Improvement of Society by Society

History.—Written July 14, 1904, to March 30, 1905. As this work and my *Pure Sociology* together constitute my system of Sociology, the most that was said in describing the origin of *Pure Sociology* (supra No. 487) applies equally to *Applied Sociology.* The provisional title-pages of both were drawn up on January 1, 1901. The prolonged task of gathering notes done in the winter and spring of that year and there described, related to the whole system, and when I began the work of referring these notes to their proper places in the system I had to prepare tables of contents of both volumes and number and name the chapters. That of *Applied Sociology* was drawn up on March 25, and the notes were all arranged by chapters for both volumes on March 31. The work of combining entries relating to the same or similar subjects was done by chapters, those of *Pure Sociology* being finished and those of *Applied Sociology* begun on April 26. In my nightly excogitations over the work while camping in the field in Arizona in May and June the chapters of both volumes were renamed, rearranged, and renumbered. It was on July 3, that I reduced the result of all this thinking to writing in the form of new tables of contents, and then the mass of notes had to be reclassified and marked anew to correspond to the new arrangement. When all this was done on July 14, and I commenced writing, it was of course the *Pure Sociology* that was then begun, and everything relating to the *Applied Sociology* lay fallow for exactly three years.

On July 4, 1904, I hunted up all my unpublished notes and manuscripts relating in any way to *Applied Sociology*, preparatory to writing the book. Many of these were what I call "inspirations," *i. e.*, thoughts jotted down at the moment they occur to me for fear that they will escape me forever (see *Pure Sociology*, pp. 442–443) which I had been collecting for many years, and of which I had hundreds. I had classified them into "used" and "not used," the former class having been incorporated into my earlier books from *Dynamic Sociology* to *Pure Sociology*. It was only the latter class that were available, but as *Applied Sociology* was to cover much of the ground traversed in *Dynamic Sociology*, the ideas of this class had not found a place in my other works and were waiting till I should return to that subject. I seemed to have completed a cycle. Starting, as it were, with applied sociology, I had left it to wander through all the mazes of pure sociology, and finally returning to my original starting point, this time with what I regarded as a far better equipment than I had at the outset.

These notes had to be treated like all the rest, marked and classified, and referred each to its appropriate chapter, section, and subsection in the new book. There was also much other matter that had to be treated in the same way. This work took ten days of the time I could devote to it.

At last, on July 14, 1904, I was entirely ready to commence writing. The following note occurs in my diary for that day. "At 3 I broke ground on my book, *Applied Sociology*, and wrote two pages. I am writing on the same paper as *Pure Sociology*. It is just three years ago to-day that I wrote the first page of that. It is not wholly a coincidence, but I was just ready to begin to-day."

While on the subject of manuscripts, and to show that all do not regard them as so trivial, and as this work completes my system of sociology and is at this writing (October 12, 1910) my last book, I will add the following account:

On December 31, 1909, while at Columbia University attending a meeting of the American Sociological Society, Mr. Gaillard Hunt, the historian, now employed in the Library of Congress, and whom I had known ever since his father, the Hon. Wm. H. Hunt, whom Mr. Garfield made Secretary of the Navy, purchased my house in Washington, approached me and asked me if I would be willing to give the manuscript of my *Dynamic*

Sociology (he did not seem to know my later works) to the Library of Congress. I replied that I thought I had made just that provision in my will, but that I would look the matter up. Soon after I received the following letter:

LIBRARY OF CONGRESS, OFFICE OF THE LIBRARIAN,
WASHINGTON, January 4, 1910.

MY DEAR DR. WARD:

My colleague, Mr. Hunt, has reported to me your intimation to him of your intention to bequeath to the Library of Congress the manuscript of your *Dynamic Sociology*. I write this word to express our appreciation of this, and also to add my hope to his that you would include in the bequest such of your correspondence relating to this work and to your other scientific activities as you may consider of interest to science, to history, and to literature.

Faithfully yours,
HERBERT PUTNAM,
Librarian of Congress.

DR. LESTER F. WARD,
Brown University,
Providence, R. I.

I took an early occasion to get my will out of the safe deposit box and examine it, and I found that on December 5, 1901, I had bequeathed the manuscripts of all my books to the Library of Congress. I have them all, *Dynamic Sociology*, *Psychic Factors of Civilization*, *Outlines of Sociology*, *Pure Sociology*, *Applied Sociology*, but they are in the form of printer's copy returned with proof and retained. I had therefore taken pains to add in the will to the word "manuscripts" the words "printer's copy." I then reported the result to Mr. Putnam and received a note of thanks from him.

I am going to mention here a still more trifling matter for those to smile at who may. On November 24, 1900, I bought a cheap stylographic pen, the Sheiry brand, at Morrison's paper store in Washington. It was very coarse, but flexible, and suited my hand, never failing to yield its ink on any occasion. I sat down the same evening to write with it my paper on *Sociology at the Paris Exposition of 1900* (supra No. 481), and wrote the whole of that. Then I wrote with it my *Pure Sociology* including *Contemporary Sociology* (supra, No. 480). I also wrote with it all the articles written in the three years between that and the *Applied Sociology*. Then I wrote *Applied Sociology* with it, and everything I have written since I have written with that same pen. I am writing these sketches with it, and the end is not yet, although the point has become a little sharp, and it has grown coarser, until it now makes a very heavy mark and consumes much ink.

The several chapters of the book were written as follows:

Chapter I, July 14–21, 1904.
Chapter II, July 22–25, 1904.
Chapter III, July 26–August 4, 1904.
Chapter IV, August 4–5, 1904.
Chapter V, August 6–12, 1904.
Chapter VI, August 12–September 3, 1904.
Chapter VII, September 6–October 13, 1904.
Chapter VIII, October 13–November 8, 1904.
Chapter IX, November 8–December 30, 1904.
Chapter X, January 2–23, 1905.
Chapter XI, January 23–26, 1905.
Chapter XII, January 26–31, 1905.
Chapter XIII, February 1–8, 1905.

The analytical table of contents was drawn up February 9–15, 1905, and the bibliography was prepared March 5–22. The preface was written March 14–30, which latter date it bears. From February 20, to March 9, 1905, I prepared a slip index of the manuscript, which is virtually the index of the book, the manuscript pages being changed to those of the printed volume as fast as page proofs arrived. Eight days (February 10–17) were spent in the search for appropriate quotations, mottoes, etc., to place at the heads of chapters, for the purpose of embellishment. This work and the *Psychic Factors of Civilization* are the only ones of my books which have this feature. It was omitted from *Pure Sociology* on account of the great size that the book had attained. I regard it as of the highest importance from both the literary and the scientific point of view. Still, very few readers ever use it, and the labor bestowed upon it, which is very great, is not appreciated by the public.

The illustrations in Chapter IX cost much time and labor, not to speak of money. With the exception of the chart (Plate V), based on Jacoby's tables, they were all adapted from those of Odin's *Genèse des Grands Hommes*, but I made extensive, and, as I think, important changes in them in the direction of improvement, simplicity, and clearness, as I also did in most of his statistical tables.

After completing the table of contents on March 25, 1905, seeing that the work was practically ready for the press, and not wishing to lose time, I wrote on that day to the Macmillan Company, describing it and offering it to them for publication. On March 29, and again on March 31, they wrote asking me to send the manuscript. On receipt of the first of these letters I did send it, viz., on the 31st. Mr. Brett, President of the Company, was away at the time, and it was referred to their adviser. On April 27, Mr. Brett wrote me a somewhat long letter to the effect that my *Pure Sociology*, though published two years, had not yet repaid its cost, and they feared this work would be in the same case. He wanted me to guarantee him against loss, but did not specify how, and returned the manuscript. Before the manuscript arrived I wrote him on April 28, that I was coming to New York the following Monday (this was Friday), and would see him in the matter, asking him not to return the manuscript. But it arrived the same day after I had mailed my letter. He wrote me the next day saying he would be glad to see me and talk the matter over, and that they certainly wanted to publish the book if they could do so without its involving financial loss. That letter I did not receive till after my return from New York and Boston, where I had made other arrangements. If I had received it I should probably have gone to see him, but I am very glad I did not, for I might have accepted an unsatisfactory solution. When the manuscript came back I was a little indignant, and I resolved not to let the Macmillans publish it if I could find any one else to do it. I had business in New York and remained there a day, but I did not go to Macmillan's. I saw some other publishers and satisfied myself that I could get the book published in New York without a guarantee. But I had always been so much pleased with the *Psychic Factors*, published by Ginn & Co., Boston, and with my relations with that house, especially with the scholarly proof readers and literary assistants, that I resolved to try them first. I therefore went to Boston, and on May 2, I saw Mr. Edwin Ginn, who referred me to Mr. Charles H. Thurber, the active man of the firm. I had with me the table of contents, preface, etc., which I showed him. He at once saw that the work was a proper one for them to publish. I told him I would not be satisfied with the 10 per cent. on the receipts that they paid me for the *Psychic Factors*, but wanted the equivalent of 10 per cent. on the retail price, as paid by the Macmillan Company and other firms. He said they would pay me 12½ per cent. on the receipts, which would be equivalent to 10 per cent. on the retail price. That I agreed to. I felt that I had achieved a triumph and returned in high glee. I sent on the manuscript May 6. They began making estimates at once. There was some difficulty about the plates, but I had fully explained them to Mr. Thurber, and all matters were arranged. The colored plates cost heavily. All was settled and the agreement was signed on June 13. There was a long delay, due to the illness of the manager of their manufacturing department, and they did not get fairly to work until September 21, when I received sample pages. Proof began to come on the 29th of that month.

I had begun the book proper with a section which I called "Personal Reflections." The publishers thought this would be more properly included in the preface, and returned the matter to me on October 2, with that suggestion. I thought the matter over and concluded to suppress the "Personal Reflections" entirely, and introduce a brief substitute. To show what these "Personal Reflections" were I introduce them into this work as part of the history of *Applied Sociology*.

SOME PERSONAL REFLECTIONS

An ill-considered word in a prominent place often leads to unpleasant consequences. The allusion to applied sociology in the preface to *Pure Sociology* was a mistake of this kind, which was rendered doubly unfortunate by the virtual promise to follow up that work with another to be called *Applied Sociology*. To judge from the number of reminders of that promise that have come from all sources, amounting to a general demand, I had almost said a clamor, it might be inferred that nobody cares for science in and for itself, and that all that is wanted is some sociological panacea that promises to cure all the ills of society.

Nothing can be more certain than that those who are looking for a book on applied sociology, intended to complete the system of sociology of which *Pure Sociology* forms the first part, are destined to be grievously disappointed, and that the present work will fall far short of their expectations. What is expected it is difficult to determine, because, from the above-mentioned indications, there is great variety in results anticipated, and some are apparently incompatible with others, so that what might satisfy some would disappoint others.

It would have been wiser in the preface to *Pure Sociology* to have said that the work was designed to form a sociological, as the *Psychic Factors of*

Civilization formed a psychological basis for *Dynamic Sociology*, the secondary title of which is *Applied Social Science*. The applied sociology is therefore already written. The most of my work since *Dynamic Sociology* appeared has been devoted to supplying the foundations for that work. Its chief defect consisted in assuming too much. In the first place it assumed that so simple a proposition as that the desires and wants of men constitute the social forces needed no elaborate demonstration. But the decade that followed its appearance abundantly proved that this was not the case, and the *Psychic Factors* was written as an answer to all the manifold objections arising from the general failure to understand that proposition. It also explained much more fully the nature of the directive agent, which was equally misunderstood Various other props were supplied by the *Outlines of Sociology*, but even then it was evident that the principal foundations of dynamic sociology were still wanting.

It was evident from the outset, and was clearly stated, that the great bulk of the extant sociology was devoted to social statics, *i. e.*, to the detailed description of human races and human institutions. Although it was not so called, this all belongs to social statics. But it is not all of social statics. It leaves out the most important part of it. It pays no attention to the principle of social statics. It shows what institutions are but does not show how they came to be what they are. The formulation and adequate illustration of this principle has proved the most difficult task thus far encountered in establishing a complete system of sociology.

The distinction between "passively dynamic" and "actively dynamic" sociology was somewhat clearly drawn, but the former was not adequately treated. This is one of the principal distinctions between pure and applied sociology. These and many other fundamental principles, considered from the standpoint of both the dynamic and the directive agent, constitute pure sociology, which does not deal with the detailed description of social structures but with the philosophic basis of social science. It is now clear, as it was not then, that both this sociological basis and also the psychological basis are essential to a full comprehension of the meaning of applied sociology.

Proof of the book began to arrive on October 4, reaching the form of pages on the 24th. Enough of the latter had been received on the 30th to enable me to commence work on the index. As already stated, I had a complete slip index of the manuscript. My plan in this, as in *Pure Sociology*, was to indicate on these slips the page numbers in red ink over those of the manuscript. In doing this, however, I drew red ink lines across the folios of the manuscript where pages began and ended, and put the numbers of the pages on the manuscript at these points, also in red ink. The manuscript, therefore, of both books always indicates the pages of the printed book. The paragraph at the end of Chapter VII, that is, of Part I, was not in the original manuscript. The printers saw that the page was short and looked bad in such a place, and they asked me if I could add some matter to lengthen it and give it a better appearance. What I wrote was something that I had thought much about and was wanting to say somewhere. It furnished precisely the opportunity I was seeking, and it occurs in the most appropriate place that could have been selected. It was written on January 3, 1906.

The contents that were prepared in February, 1905, consisted of a synoptical and an analytical table of contents. The former seemed superfluous, and the latter was defective. I therefore suppressed the synoptical table and rewrote the analytical one, calling it simply "Contents." This was begun January 27, 1906. It gave the printed pages only and could be written only as far as these were received. This and the indexing therefore went on *pari passu* with the receipt of final proofs. On February 23, I sent on as much as I had been able to do to that date, and on February 27, I sent the balance, the whole of the text proper being in pages. It was on February 23, too, that I commenced preparing the manuscript of the index, which was finished on March 1. On April 14, the last proof was received, corrected, and returned. The preface as originally written did not include the last paragraph relative to Odin's *Genèse des Grands Hommes*, and after consultation with the publishers this was written on April 18, 1906. On April 21, I sailed for Europe. The book was published on July 2, and I received an unbound copy in Paris on the 9th and bound copies on the 13th.

Among all the letters that I received from those to whom I sent the book I will select a single one, that of Dr. De Greef, who also wrote me a glowing acknowledgment of my *Pure Sociology*. But this one is so extravagant that there is no danger that anyone will take it literally. Certainly no one who knows his works will accept the judgment here expressed with regard to them.

BRUXELLES, BELGIQUE
Le 1er Octobre, 1906.

CHER COLLÈGUE:

Je viens de finir pendant les vacances la lecture de votre "Applied Sociology." C'est le complément magnifique de votre "Pure Sociology." Après avoir lu ces deux volumes, mon impression est que je devrais jeter au feu tout ce que j'ai publié en Sociologie. Nous avons suivi des voies en partie différentes; je commence à croire que c'est vous qui avez suivi la bonne, et que le seul service que j'aurai rendu en suivant la mienne sera d'éviter à ceux qui suivront de commettre les énormes erreurs. Votre œuvre est remarquablement belle; permettez-moi de vous en féliciter et d'espérer que j'aurai encore le plaisir de vous voir.

Votre bien devoué

G. De Greef

Portions of other important letters were used, with the consent of the writers, by the publishers in their announcement of the book (see infra, No. 524), but besides Professor Thorndike's review in *Science* he wrote a much longer one for the *Bookman* (New York, Vol. XXIV, No. 3, November, 1906, pp. 290–294). He finds a good deal of fault with my doctrines. Professor Carl Kelsey reviewed the book very sympathetically in the *Annals* of the American Academy of Political and Social Science for November, 1906 (Vol. XXVII, No. 3 [whole No. 97], pp. 487–489 [129–131]). The fullest and most unqualifiedly favorable review that appeared at all was that of Professor George Elliott Howard in the *American Journal of Sociology* for May, 1907 (Vol. XII, No. 6, pp. 854–859). It was a surprise to me, for I did not before know that Dr. Howard was so favorably disposed toward my views. Dr. Ross's review in the *Political Science Quarterly* for June, 1907 (Vol. XXII, No. 2, pp. 256–358) was of course calculated to produce a fine impression. Professor Adolfo Posada reviewed the work at length in *La Lectura*, Madrid, for October, 1906. He has treated it still more fully in his later book, *La Sociología*.

The socialists who believe in education are taking up *Applied Sociology* as a sort of guide and hope in their cause. Mr. George R. Kirkpatrick, a prominent socialist lecturer, organizer, and author, has espoused it vigorously, and wrote a long laudatory letter to the publishers about it, which they brought to my attention. He has also written several letters to me in the same strain. Many socialist circles have made the book the basis of their readings and discussions, notably one conducted by Mr. J. Lebovitz of the Library of Congress in Washington. It is to be hoped that socialists and workingmen may come more and more to see that their hope is in true education.

On May 4, 1906, I spoke to M. Brière about publishing a

French translation of *Applied Sociology*, and he expressed a willingness to do so. I had written to M. Weil about translating it, and on July 17, I received a letter from him saying that he was ready to begin the translation. He came to see me on May 31, and said he had seen M. Brière who was rather indifferent about publishing it. He was not willing to pay him enough for translating it. I gave him the unbound copy to make the translation from. After I returned to America, namely from September 26, to October 8, I spent much time copying from the original sources all the French passages that I had translated into English, so that he would not translate them back into French, and on October 9, I sent these all to M. Weil to use in his translation. On November 9, I received a letter from him saying that he had been made secretary to a certain lawyer and his duties would not permit him to undertake so large a piece of work outside. He offered to turn the copy of the book and the manuscript material I had sent him over to anyone who would undertake it. I made no further efforts, and these documents are still in his possession.

On October 23, 1905, Mrs. Unger wrote me that she wanted to translate *Applied Sociology* into German. In order that she might gain time I sent her the galley proofs of the six chapters that were then in type, and from that time on I continued to send her proof, either galley slips or pages, until the book appeared, which of course she received. She translated most of it from the proofs, finishing Part I on February 11, 1906, and keeping up somewhat closely as they were received, but the work was partially suspended for a while to begin a translation of Gumplowicz's *Rassenkampf* into English. It was resumed in the fall and finished in the spring of 1907. It was supposed that the Wagner firm in Innsbruck would publish the German translation. Schumacher had told me so in 1906 when I was there. Both Mrs. Unger and I explained the whole matter to Professor Gumplowicz, and he proposed, of his own accord, to revise the manuscript before it went to press. Mrs. Unger sent it to me in instalments during the summer of 1907 and I revised it thoroughly. I made notes of all the German passages, hunted them up, making several trips to Boston for the purpose, and copied them, and they were incorporated in the manuscript.

Gumplowicz had dedicated to me the third enlarged edition of his *Allgemeines Staatsrecht*, Innsbruck, 1907, in these words:

"Meinem Lieben Freunde und Kollegen Lester F. Ward, Professor der Soziologie an der Brown Universitaet in Providence (Rhode-Island), Praesident der Amerikanischen Soziologischen Gesellschaft, zur Erinnerung an unsere soziologischen Debatten in Graz im August 1903 und im Juli 1906—Gewidment."

In view of this and of his great kindness in correcting the proofs of the *Reine Soziologie*, and his offer to revise the manuscript of the *Angewandte Soziologie*, I decided to dedicate the latter work to him. I wrote the English draft of my dedication on April 24, 1907, which was in these words:

"To my Honored Friend and Colleague, Dr. Ludwig Gumplowicz, Discoverer of the Principle, as distinguished from the Law, of Social Evolution, which is to Sociology what Natural Selection is to Biology, I gratefully dedicate this volume."

This was sent to Mrs. Unger on the same day it was written and translated into German by her. The following is her translation:

"Meinem geehrten Freunde und Kollegen, Dr. Ludwig Gumplowicz, dem Entdecker des Prinzips, als unterschieden vom Gesetz der Sozialen Evolution, welches für die Soziologie ist, was die Natürliche Zuchtwahl für die Biologie ist, ist dieser Band voll Dankbarkeit Gewidmet."

On April 29, I wrote to Gumplowicz and told him I had dedicated the book to him. My revision of her manuscript was made from August 7, to September 8, 1907. The special library research mentioned above was done October 14–20. I found proofs of the maps and chart and took them with me when I went to Madison at the end of December where Mrs. Unger was to be in attendance at the meeting of the American Sociological Society, and handed them to her there to translate the lettering and embody them in her manuscript.

On January 11, 1908, negotiations were begun looking to the publication of the book. I wrote to both Schumacher and Gumplowicz on the subject. On February 10, I received a letter from Schumacher who wished to wait till he knew what success the *Reine Soziologie* was meeting with. Mrs. Unger sent the manuscript to Gumplowicz and he commenced revising it. Some time after this Schumacher wrote to Mrs. Unger that he could not publish the *Angewandte Soziologie* because the *Reine Soziologie* had proved a failure (see supra, No. 487). I have no doubt that if Gumplowicz had retained his health he would have found another publisher. About the last work he did was on this book.

November 12, 1906—Ætat. 65.

524. [Second Announcement of Applied Sociology]

History.—Written September 16 and 17, 1905. [See supra, No. 521]. I went to Boston on September 18, 1906, taking with me a number of letters I had received containing expressions favorable to the work, and arranged to have these printed in a circular announcing it. The writers were communicated with either by me or by the publishers and their consent obtained. The following are the extracts used, and I have preserved the letters:

LETTERS IN PRAISE OF APPLIED SOCIOLOGY

EDWARD A. ROSS, Professor of Sociology, University of Wisconsin

This work puts the author so far above other social philosophers that I doubt if henceforth in any quarter his leadership will be disputed. To my mind the author's reverence for the brother man, his refusal to depreciate him, his insistence on his potential worth and capacity, are even a clearer mark of greatness than the compass of his knowledge and the depth of his thought. His exposé of the bias and pride of intellect current among scholars and scientific men carries the war against unreason into a new quarter and puts on the defensive a class that has supposed itself invulnerable.

There is no question that Dr. Ward is the greatest authority on fundamental social science in the world to-day, and I regard Applied Sociology as his crowning achievement. I shall adopt it as a text next year, for his conceptions of how society may accelerate its own progress are so much ampler and higher and better based than those of any other social philosopher that his work is absolutely without a fellow.

ANDREW D. WHITE, Lecturer in Cornell University

Applied Sociology impressed me strongly as a most important contribution to the subject of which it treats. Some of the chapters have so greatly interested me that I have thought much upon them.

George E. Howard, Department of Political Science, University of Nebraska

I am delighted with Ward's Applied Sociology. It is a worthy conclusion of the author's magnificent system of sociology. Aside from its first-rate importance as a contribution to the science which Dr. Ward has done most to create, the work is of fundamental interest to education. No other book has done so much to reveal the real function of knowledge. It should be read by every teacher. I shall make use of it in my classes.

Charles A. Ellwood, Department of Sociology, University of Missouri

I find myself in hearty agreement with your general argument. You have laid us younger men in sociology under heavy obligations to you by your many epoch-making contributions to our science. I cannot but congratulate you and ourselves upon this completion of your system of sociology.

S. N. Patten, Professor of Political Economy, University of Pennsylvania

I have been reading your new book with great interest. I think it will prove the most effective book you have written. There is a clearness and force that must tell with the public.

C. W. A. Veditz, Professor of Political Economy, George Washington University, Washington, D. C.

I regard the book as a chef-d'œuvre of the very first magnitude. It is not only a great book, but it is the kind of book which is needed just now. Sooner or later I shall force all my students to get hold of it, and once they get hold of it I defy them to drop it until they have read it through, provided they have any red blood in their veins or any gray matter in their craniums. As I read page after page I find I can scarcely keep from shouting with delight.

A REVIEW FROM

SCIENCE

SEPTEMBER 7, 1906

The clearness, brilliancy, and vigorous defense of some pronounced doctrine which we have learned to expect from Professor Ward are characteristic of this book. It concerns real facts, not verbal distinctions; it delights by its cleverness of thought and style.

Edward L. Thorndyke
Teachers College, Columbia University

The announcement is as follows, and is the same as that issued January 24, 1906, but it was stitched with all these testimonials into a little booklet.

APPLIED SOCIOLOGY

A TREATISE ON THE CONSCIOUS IMPROVEMENT OF SOCIETY BY SOCIETY

By Lester F. Ward, Professor of Sociology in Brown University

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8vo. Cloth. 384 pages. With charts. List price, $2.50

THE purpose of this new work on sociology is to point out how the obstacles to human progress may be overcome by the application of the principles of social science, and how the human race may be brought into the full fruition of its powers.

It is maintained that the agents of history are men, that human progress is due to the actions of the members of society, or, in other words, that civilization is the product of human achievement. But it is clear that only a comparatively few contribute to this end, and that the result is proportional to the number and efficiency of those so contributing. The problem therefore is how to increase the number and efficiency of the agents of civilization.

It is further shown that the actual number and the actual quality of these agents are capable of being greatly increased and improved by the removal of unfavorable conditions.

The argument is supported by historical facts, and the work is to a considerable extent statistical. It is illustrated by colored maps, charts, and tabular exhibits.

November 12, 1906—Ætat. 65.

525. [Announcement of the Psychic Factors of Civilization, Second Edition]

History.—The Second Edition of this work appeared almost simultaneously with *Applied Sociology* from the same house, and it was thought best to have the circular announcing the latter (see supra, No. 524) include both. This announcement is practically uniform with the first part of the third announcement of the First Edition (see supra, Vol. V, No. 351).

THE PSYCHIC FACTORS OF CIVILIZATION

Second Edition

By Lester F. Ward, Professor of Sociology in Brown University

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DEPARTMENT OF SPECIAL PUBLICATION

8vo. Cloth. xxi–369 pages. List price, $2.00

THIS work is an original contribution to both psychology and sociology, and is, in fact, a combination of these two departments of science. It is the first attempt that has been made to show in a systematic way the workings of mind in social phenomena. It has hitherto been customary with those who recognize the operations of law in human affairs to compare them with those taking place under the dominion of vital forces. Sociology has been made a department of biology. Society has been treated as a living organism, and the laws of production, distribution, and consumption have been likened to the process of nutrition, circulation, and assimilation. Political economy has thus gained the name of "the dismal science" because it has been treated as mindless and soulless. Over against this purely *physiological* economy we now have fully set forth in this book a *psychological* economy, a philosophy of mind as the primary motive power of the world in all things above the level of animal life.

The work is divided into three parts,—the subjective factors, the objective factors, and the social synthesis of the factors; and appeals especially to the following classes of readers: psychologists, sociologists, economists, philosophers and thinkers, biologists and naturalists, also to socialists and to social and political reformers.

December 7, 1906—Ætat. 65.

526. [Note on Sympodial Plants]

History.—Written November 11, 1906. No response was ever received from this note. Botanists know nothing whatever about the subject. I have erected it into an important sociological principle (see *Pure Sociology*, pp. 71–79), which I now illustrate to my classes by linden twigs, but I would like to know of other good examples besides these and the grapevine. I use these for the cincinnate, and borage plants for the bostrycoid form.

On July 15, 1911, Mrs. Emily Palmer Cape discovered and brought to my attention the fact that the common sumac, *Rhus glabra*, is sympodial in its growth, and good examples of it were obtained for use in illustrating my lectures on the subject at the Summer Session of Columbia University which I was giving at that time.

Science, N. S., New York, Vol. XXIV, No. 623, Dec. 7, 1906, p. 749.

MR. LESTER F. WARD would be glad to be informed of as many as possible of the native plants, especially trees and shrubs, of the country, which possess the sympodial type of branching, either complete or partial. Names, lists, notes, etc., may be sent to him at Brown University, Providence, R. I. Observations of sufficient importance might be sent to *Science* for publication. Any estimates of the proportion of sympodial to monopodial plants would be of interest. The subject is a neglected one, and references to the best treatment of it would be useful.

December 28, 1906—Ætat. 65.

527. The Establishment of Sociology.—Abstract

History.—Written December 1, 1906. I received a letter dated November 27, 1906, from Professor Henry B. Gardner, Secretary of the Committee of Arrangements for the annual meetings of the various associations in Providence, requesting me to furnish an abstract of my address as president of the American Sociological Society, and asking for three copies of the same to furnish to each of the local newspapers. I wrote it and had seven copies made, four of which I sent to Professor Gardner. I think the Providence *Journal* was the only one that printed it. A portrait accompanied it. (For the address in full see infra, No. 530.)

The Providence Journal, Vol. LXXVIII, No. 311, December 28, 1906, p. 2.

January 4, 1907—Ætat. 65.

528. Review of Sociology and Social Progress: A Handbook for Students of Sociology. Compiled by Thomas Nixon Carver, Ph.D., LL.D., David A. Wells Professor of Political Economy in Harvard University. Boston, Ginn and Company [1906]. Pp. vi+810; 8°. List price, $2.75; mailing price, $2.95

History.—Written December 6–9, 1906. On October 15, 1906, Dr. Cattell asked me if I would review the book, which I agreed to do, and on October 23, I received it. I read it in three days, but had to lay it aside for over a month. I wrote the rough draft on December 6, and the final draft on the 9th. Proof was received on December 19.

Science, New Series, New York, Vol. XXV, No. 627, January 4, 1907, pp. 27–28.

March 7, 1907—Ætat. 65.

529. Mars and its Lesson

History.—Written January 27, to February 2, 1907. I had always been interested in the planet Mars as the possible abode of life, or even of intelligent beings, having alluded to it in my *Psychic Factors of Civilization* (p. 256) in 1893. On June 4, 1901, when I had brought my outfit to Flagstaff from the Little Colorado Valley [see supra, No. 479] in order to ship the collections from there, which required several days, I was invited to go out to the Observatory in the evening. Professor A. E. Douglas rode up with me. Professor Percival Lowell met us there and they insisted on my taking a long look at Mars. It was not an exceptionally good presentation, but the sky was clear and the planet was high above the horizon.

> "I was surprised at what I could see. The south polar cap of snow was perfectly clear and I could see great canals or long cavities in various directions. They made me draw what I saw and sign my name to it."

The words just quoted are from my notebook written the same evening.

The subject continued to interest me, and in my paper on *Social Differentiation and Social Integration* (supra, Nos. 489, 490), as well as in *Pure Sociology* (pp. 470–471), I speculated upon it. On January 7, 1907, Professor Lowell gave a lecture on Mars at Brown University which I attended. I met him before the lecture and reminded him of my visit to his Observatory in 1901, which he remembered. The lecture was of course intensely interesting, and having nothing else in hand just at that time, I resolved to read up the subject as far as I could. Lowell's *Mars and its Canals* was then just issuing from the press and I obtained and read that first. *Mars and its Mystery*, by Professor Edward

S. Morse, appeared about the same time, and I ordered that and read it. I saw that I had missed Lowell's earlier work on *Mars*, 1895, and I secured that and read it. I want to say of Morse's book that I saw in my one and only sitting above described about as much as he saw on June 5, after more than twenty sittings and with a much better presentation of the planet. See his figures for that date on plate IX, facing p. 162 of his book. My drawing more closely resembles his figure for June 9–10, but I did not see the isolated markings that he there represents. Professor Lowell has kindly sent me a tracing of the sketch that I made.

On January 20, 1907, I wrote to Dr. Cattell offering to review the books of Lowell and Morse for *Science*. On January 24, he replied that he had given out both the books for review, but would be glad of an article from me on the subject for the *Popular Science Monthly*. On January 21, Mr. H. L. Koopman, Librarian of Brown University, wrote to ask if I would contribute an article to the *Brown Alumni Monthly*. I concluded to write something on Mars, and having carefully read and annotated all the above-named books, I commenced on January 27, a rough draft of an article. On the 28th I drew up the Martial "cosmic day" of 96,000,000 years, allowing 24,000,000 for the Psychozoic period. On the 30th I saw Mr. Koopman and agreed to let him have the article, and that evening I virtually completed the outline of it. The next day I took Mr. Koopman both the dials and asked him if he could have them drawn. He turned them over to Mr. J. L. Wheeler, Second Assistant Librarian, and who was also in one of my classes, and he did the work in a very satisfactory manner. The dial of the earth appears in this article to better advantage than in any of the previous ones. In *Pure Sociology* (p. 40) it is so reduced that the lettering is scarcely legible. The dial of the Martian day is equally fine.

The original draft of the article was too long for the *Brown Alumni Monthly*, as in writing it I had designed it for the *Popular Science Monthly*. It had to be cut down to the available space and the final draft written. I had prepared a short bibliography enumerating the principal books used in preparing it, but that, too, had to be sacrificed to space, which gives it too much the appearance of having been "evolved out of my inner consciousness." But such things are noticed only by the writer. Proof was received on February 25, and it appeared on March 7.

I sent a copy of the *Brown Alumni Monthly* containing this article to Professor Lowell, and on April 1, I received the following letter:

LOWELL OBSERVATORY, FLAGSTAFF, A. T.
March 26, 1907.

PROF. LESTER F. WARD, LL.D.
Brown University, Providence, R. I.

DEAR SIR:

There has been sent me your excellent article in the 'Brown Alumni Monthly' on 'Mars and its Lesson' which I greatly enjoyed reading. It is among the first fruits of what must steadily increase: the help afforded the study of our own Earth by the study of Mars. Your valuation of the well-named psychozoic era is striking and important and, as you say, probably underestimated rather than overestimated. In this connection—as they were along these lines, sketching what I may term planetology, or the development of worlds as such—I could wish that you had heard my lectures last winter, but as they are to appear in the *Century Magazine* beginning next November, you will be able to see them there.

Believe me with appreciation of your memoir,
Very truly yours,
PERCIVAL LOWELL.

As to the last part, I hardly need say that I read all the articles on *Mars as the Abode of Life* as fast as they appeared and kept them together for reference, but they also soon appeared in book form. I have also read Professor Lowell's *Evolution of Worlds*. His style is so charming and attractive that no one can refrain from reading everything he writes.

I also received the following delightful letter from President Faunce:

PRESIDENT'S OFFICE, BROWN UNIVERSITY,
PROVIDENCE, March 26, 1907.

DEAR PROFESSOR WARD:

I cannot refrain from thanking you for that most admirable and stimulating article in the last number of the *Brown Alumni Monthly*. It has been widely copied in different parts of the country, and will certainly widen the universe to any man who reads it. How petty seem our griefs and angers and strifes in the presence of the æons which you have suggested. I regret every week that Astronomy has been so side-tracked in our university curricula that very few students are taking it. It would save us all from pettiness if we could occasionally look through the telescope as you have urged us to do in the *Monthly*.

With kind regards, I am
Sincerely yours
W. H. P. FAUNCE.

The statement in this letter that the article was being widely copied by the press was very true. No article of mine ever received so many notices. I of course did not see all, but the New York *Tribune* of March 12, gave it a column, the New York *Globe* of the 16th reproduced nearly the whole of it, including both dials, the Springfield *Republican* of March 17, the most of it, and many other papers noticed it immediately. On April 12, Mr. F. W. Coburn of Boston, representing a large syndicate of newspapers, called on me with a letter of introduction from President Faunce, and asked me to supply him with data for a syndicate article on Mars. I gave him the chief points and supplied him with a number of documents. On the 17th he brought me the article for revision and approval, and I looked it through and told him to go ahead. It occupies about three columns of most newspapers and appeared in twelve prominent papers. Mr. Coburn gave me a list and I obtained copies of most of them. The article was always the same but the headlines varied. This article received at least one prominent notice abroad. It was reviewed in the Pall Mall *Gazette* for April 5, 1907, over the initials "C. W. S.," which I ascertained to mean Prof. C. W. Saleeby. He did not venture any serious criticisms, and accepted the theory as a legitimate one.

The editorial in the *Brown Alumni Monthly* in which the article appeared, though unsigned, is from the pen of Mr. Koopman, and should be given here entire to complete its history. It is as follows:

THE EARTH'S FUTURE

We present our readers this month with an article from the pen of Professor Ward in which are set forth conclusions of enormous significance to the human race. Hitherto man has lived under the shadow of impermanence. So fixed has been his belief in the ephemeral character of the world he inhabits that he has time and again set the date of its destruction; and though the years 1000 and 1843 and 1881 have all been safely passed, still he has a sense of living at the end rather than at the beginning of the ages. The religions are full of this conviction, and both science and history teem with the depression and perversion engendered by this pessimistic instinct. But now a cheerful optimist comes forward and tells the race that it has hardly chipped the shell, that the men of today are living in the feeble dawn not so much of civilization as of existence itself. With twenty-four million years still to the good, what may not the race accomplish that has achieved so much in the six thousand years of recorded time, which prove to be but seven seconds of the cosmic day! The thought means a transformation of all sociology, philosophy, poetry, religion. It means that man is "the heir of all the ages, in the foremost files of time" in a vast and wonderful sense whereof he never has dreamed. It means that his line stretches far beyond the fabled crack of doom to which Banquo's reached, into a period which, as Professor Ward justly says, represents to our finite conception nothing less than eternity. Next summer, when

"the first watch of the night is given
To the red planet Mars,"

these are the glad tidings which Professor Ward's epoch-making thought would have us rejoice in as we contemplate the ruddy orb aglow in the southern heavens.

Perhaps I should not leave this subject without giving some account of other work that I have done along this line, but which is still incomplete, or rather in embryo, and which may never be published or completed. I had in mind from the first something more extensive, and as early as February 19, 1907, or while the article was in press, I commenced writing on what I called *Reflections on the Planet Mars*. I put as my motto at the head the following quotation from Professor Lowell's first book on Mars (p. 211): "That Mars seems to be inhabited is not the last, but the first word on the subject." My idea was, as he expressed it in his letter, "the help afforded the study of our own earth by the study of Mars," or as I put it in my article, the "lesson" of Mars. I prepared a very complete table of the elements of both Mars and the earth in parallel columns. This was to stand at the beginning as the common source from which everything was to be drawn. After some general remarks I wrote a short chapter on "Martian Facts," and I wrote a second chapter, consisting of only two folios, entitled "Legitimate Inferences." This was followed by a third of about the same length, called "Reflections on the Facts and Inferences." There I stopped. The whole manuscript contains only 13 folios of ordinary letter paper, coarsely written, or about 2500 words. I did no work on it after February 22, and, indeed, have never done any more on this article.

The subject, however, lay on my mind and I read all I could find about it. There gradually rose in my mind the idea of some time writing a book on *Mars and the Earth*, carrying out all the numerous suggestions to their extreme logical conclusion. It was to be of such a character as to be independent of the truth of the inferences relative to the nature of Mars, so that the conclusions would be just as true whether the facts were established or not. In other words, it would say what would be if the facts were as supposed, and I could see that such a train of thought would lead to some most interesting results, and teach us much about our own earth and about ourselves. I even went so far as to mention my proposed possible book in a letter to Professor Lowell, written March 31, at which time I sent him some of my papers in which I had expressed my interest in Mars, as well as my *Petrified Forests of Arizona* (see supra, No. 436). To this letter I received a most cordial answer dated April 10, in which he says:

> "It is a great pleasure to hear that your coming work on the subject will take the dimensions of a book—the subject most certainly deserves it."

I found a few moments to work at it on September 11, and formulated the title of the book as *Mars and the Earth*, but I could not go on then and allowed it to lie untouched till July 14, 1908. On that day I began a pencil sketch of what I called

> "the Contents of my book, which I now provisionally call Ares and Gaea, Mars and the Earth, the two Life Centers of the Solar System. The Contents is really a summary of the whole book systematically drawn up."

I actually drew up the contents on that date as well as the title-page. The contents are as follows:

CONTENTS

- INTRODUCTION
- Chapter I.—Mars and the earth compared and contrasted
- Chapter II.—Natural history of a star
- Chapter III.—Solar systems
- Chapter IV.—Our Solar System
- Chapter V.—Planetology
    - Section I.—A perfect planet
    - Section II.—Natural history of a planet
        - 1.—Geology
        - 2.—Areology
- Chapter VI.—Biology
    - Section I.—Plant Life
    - Section II.—Animal life
- Chapter VII.—Psychology
- Chapter VIII.—Ethnology
- Chapter IX.—Sociology
    - Section I.—Politics (civology)
    - Section II.—Culture

I spent the better part of four consecutive days on this work and practically completed the contents, which are highly analytical and go much into detail under all the heads, especially those of Planetology, Biology, Psychology, Ethnology, and Sociology. The whole makes forty-one folios, of which Sociology occupies twenty, and is very full, losing the character of a summary and becoming in places a discussion. I never intended to have it appear in that form, but I wanted to say certain things and took that occasion to do so.

On July 28, I went to the library and read Professor Douglass's article in the *Popular Science Monthly* for May, 1907, in which he ascribes the finer lines to optical illusion. I also read Poor's *Solar System*, and was much influenced by Lowell's description of similar markings on Venus and Mercury. I was aware that most astronomers were skeptical on the whole subject, and as there was to be in 1909 the finest presentation of Mars that had ever occurred or that would occur for many years, I concluded to suspend work on the subject till there should be something like a consensus of opinion. That day has not yet arrived.

Brown Alumni Monthly, Providence, Vol. VII, No. 8, March, 1907, pp. 159–165.

THE precise knowledge now in possession of the world relative to the true nature of Mars has for its chief value to open up an illimitable field of inquiry into the most important of all problems, the rôle of life and mind in the universe. The facts are for the most part now established. The question turns entirely upon their interpretation. True, they have already been interpreted in their main features, and no logical mind can doubt the substantial accuracy of this interpretation. We must start from it as we start from the facts themselves. But every avenue thus opened branches and dichotomizes, forming a system of canals of thought as intricate and yet as rational as the system of Martian canals.

It is not proposed to reiterate here the conclusions already reached and given to the world. It is proposed to follow the reasoning farther and to search for wider and deeper interpretations which shall ultimately teach us the lesson of Mars.

The first step in this quest must be the study of *areology*, using this term in exactly the same sense in relation to Mars as *geology* is used in relation to the earth—the geology of Mars. This aspect has not of course been overlooked by writers on

Mars, but from conservatism and other motives its treatment has been inadequate. Attention has been called to the fact that, being a smaller planet than the earth, Mars would cool and hence age more rapidly. It has also been shown that on the earth the land has long been encroaching upon the sea, and that the absence of seas on Mars may be due to a continuation of this process. The gradual absorption of both the water and the atmosphere is also frequently referred to, and must be accepted as a fact. Nor has that other fact been wholly neglected, that, from its position in the solar system exterior to the earth, Mars must have taken planetary form much earlier than the earth, and is therefore, absolutely as well as relatively, a much older planet. Still, it must be admitted that much of the reasoning seems to be based on the assumption of a simultaneous start for the two planets in the race. This is so far from being true that it demands special emphasis.

The entire history of a planet may be divided into three periods. The first period extends from the time when it is thrown off from the parent body to that at which its crust has formed and the temperature of its enveloping waters has fallen to, or somewhat below, the boiling point. The second extends from this latter date to that at which either its waters or its atmosphere or both have been absorbed and the planet has been wholly converted into solid matter. The third embraces the remainder of its existence.

We can form some conception of the length of the first period from the condition of our solar system. All the outer planets except the Asteroids and Mars are still in that period. Of the second period we know much more because it is the one to which our earth has attained. Mars is also still within it. It is probable that both Venus and Mercury are already close upon this phase. As to the third period, we are in complete ignorance of its duration. To all appearances it is eternal, though the human mind refuses to accept this view.

Of these three periods in the history of a planet we are at least safe in saying that the second or intermediate one is much shorter than either the first or the last. It forms relatively but a brief span in the vast cycle of ages. Nevertheless this is the stage in which all our interest centres, and for us it constitutes the life of the planet. And here we use the word *life* in its literal sense, for this is the period which embraces all life. In the case of the earth it also embraces the greater part of all that is treated as geology, and in the case of Mars it does the same for areology as I am using the term. Clearly to grasp this last science it is necessary to attend first to our own planet and glance briefly at those aspects of geology which throw light upon areology.

The most important fact connected with our earth at the beginning of this period was the origin of life. Whatever theory we may adopt as to the particular way in which life began, it is at least certain that in some way it did begin about as soon as the conditions of the earth's surface became such as to permit its existence. And we may well accept Professor Lowell's view, shared by many others, that for the *origination* of life much higher temperatures were required than those which commonly prevail at the present time, although, after having once originated, owing to the slow rate at which the planet cooled, it easily adapted itself to much lower temperatures.

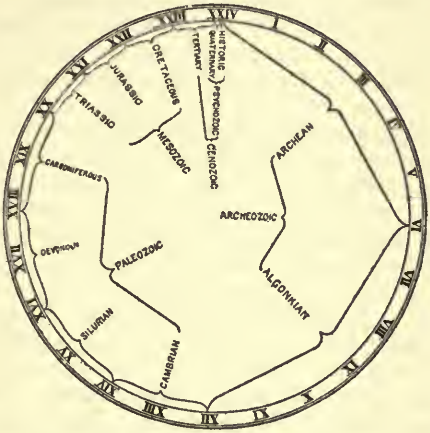
At this inchoate period there was no land, and all life was aquatic. There is probably still as much aquatic as terrestrial life, and the process of adaptation to terrestrial existence as the land areas rose and expanded was also a natural one. The greater diversity afforded and required by land life was favorable to development, and we accordingly find the highest types of life inhabiting the land and breathing air.

At present we are chiefly concerned with the length of the life period. Three classes of scientific men have discussed this question, viz., astronomers, physicists, and geologists. They have of course differed widely, but there has been a tendency toward substantial agreement. While most estimates make it at least 100,000,000 years, there are more conservative ones, even by geologists. Professor Haeckel, adopting the figure last mentioned, has proposed to call this life history of the earth its cosmic day, consisting of twenty-four hours, and he proceeds to reduce the several geological formations to this scale and to express them in hours, minutes, and seconds. The results are very striking. In other papers I have adopted his method, but it seemed best to use a somewhat lower estimate of the time, and 72,000,000 years seemed to be a fair mean to employ. The following table shows the results:

Geologic Periods	*Years*	*Hrs.*	*Min.*	*Sec.*
Archean	18,000,000	6		
Algonkian	18,000,000	6		
Cambrian	6,000,000	2		
Silurian	6,000,000	2		
Devonian	6,000,000	2		
Carboniferous	6,000,000	2		
Triassic	3,000,000	1		
Jurassic	3,000,000	1		
Cretaceous	3,000,000	1		
Tertiary	2,675,000		53	30
Quaternary	300,000		6	
Historic	25,000			30
Total age of the earth	72,000,000	24		
Age of written language	6,000			7⅕

The extraordinary fact comes out that the human race, which probably had its origin in the Quaternary period, or possibly in late Tertiary time, has only existed between six and seven minutes of this cosmic day, while the extreme maximum estimate of the historic period does not exceed 30 seconds, and that of its recorded annals is a little more than seven seconds!

Striking as this result is, as thus presented, I thought it might be rendered still more so by exhibiting it in the form of a dial, and I accordingly prepared one which has been several times published. But it will bear reproduction, and I introduce it again here for comparison with parallel results which flow from a similar study of Mars, upon which we are now prepared to enter.



DIAL OF THE COSMIC DAY OR LIFE HISTORY OF THE EARTH

The term "Psychozoic age" was proposed by Prof. Joseph LeConte, to denote the age of man, and was made by him to begin with the Quaternary or Pleistocene period, which contains the oldest authentic remains of man. It is therefore the age in which we live, and may be supposed to continue as long as man shall inhabit the earth. But the term was wisely chosen, and is much better, for example, than *Anthropozoic*, used by some authors, because it applies to any psychic or intelligent being, though it be not man. It can thus be applied to Mars regardless of the nature of the intelligent being that we now know to inhabit that planet.

We perceive from the above table and dial that the Psychozoic age has only just begun upon the earth. The psychic or intelligent being man has but just arrived on this planet. How is this in the case of Mars? In the history of the solar system Mars left the central mass ages earlier than did the earth. How long was the interval between these two events? Mars is about fifty million miles farther from the sun than is the earth. At the time that it swung off the central mass had a diameter of 283,000,000 miles. By the time the earth was ready to be detached it had shrunk to 186,000,000 miles. To know how long this period was would require a knowledge of the rate at which a nebula contracts. This knowledge is not possessed by man. It probably diminishes rapidly with the diminution of the mass, but any one can see that the intervals between the births of the planets must have been immense. In venturing therefore to estimate the difference of age of Mars and the earth the chief danger lies in making it too small. In the scheme which follows I have placed it at 24,000,000 years, or a period equal to the entire Paleozoic age of the earth. This is probably much too small, but even thus the results, as we shall see, are astonishing. This period must be added to the age of the earth to obtain the age of Mars. This assumes that the time required to reach the life stage was the same for both planets. But if, on account of its smaller size, Mars reached that stage much earlier, this would only increase the difference in their ages.

I have also assumed that the geologic time and length of the formations were the same on the two planets down to the time when Mars was in the same state that the earth is now in. This might be supposed to be less on account of the smaller size of Mars. But although the rate of contraction and the conditions of temperature would be thus affected, I am unable to see that size could have any influence upon the development of life, the formation of limestones, the erosion of the surface, or the deposition of sedimentary beds. We may suppose, then, that the history of the two planets was practically the same for the first 72,000,000 years, or to the end of their Tertiary period. To this then we must add for Mars the time that has elapsed since it reached this stage, viz., the 24,000,000 years which represents the difference in the ages of the two planets. We thus have for the age of Mars 96,000,000 years. Calling this the cosmic day of Mars and dividing it into 24 hours, each hour will represent four million years instead of three million, as in the case of the earth.

Now we must apply the geological principle of uniformitarianism to all our calculations, and assume that Mars passed through substantially the same stages as the earth had done; that it, too, millions of years earlier, had its origin of life in the then uniform aqueous envelope that surrounded it; that later, as its crust formed and crumpled to fit the retreating interior, land and land life appeared, mountains rose and were worn down, building broad continents consisting of sedimentary deposits of great thickness; that these were each filled with the remains of the life characteristic of each great geologic epoch; and that thus, in the same manner as in the earth, the Archeozoic, Paleozoic, Mesozoic, and Cenozoic periods succeeded each other, having substantially the same length as those of our younger planet.

This exact parallelism continued to the end of the Tertiary period and a short distance into the one that succeeded this. We have now the best reasons for supposing that at the beginning of this Quaternary period, as upon the earth, some one of the manifold forms of life on Mars developed a thinking faculty far in excess of all others, which gave it dominion over the rest and ultimately over its physical conditions as well. Thus was ushered in the Psychozoic age on Mars, upon which the earth is, as we may say, but just entering. From this point on, and during the vast eons that separate that remote period from the present, the parallelism ceases, and we have nothing on our planet with which to compare that portion of Martian history. We stand appalled before those 24,000,000 years, and what the course of this strange history can have been.

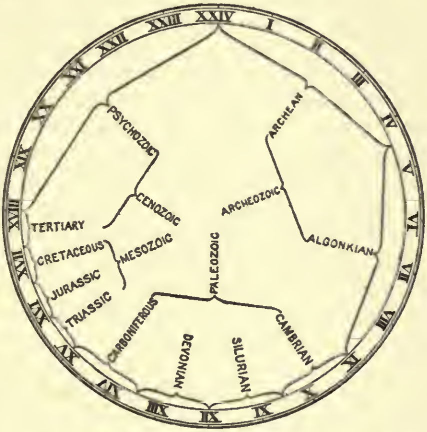
It is to the unveiling of this "mystery" that the recent revelations of the areographers have been pointing. Keeping constantly in mind the present state of our earth, and remembering that it represents the state of Mars at the corresponding period in its career, we perceive that during that time all the irregularities of the surface of Mars have been removed and it has become a smooth orb. We also learn that the greater part of its water has disappeared. But two prominent features still remain. Mars still shows evidence of vegetable life and psychic life. Considerable portions of its surface are occupied by dark areas which can only be interpreted as due to vegetation, and the whole planet is covered with a network of dark lines having dark spots at their intersections, which can only be interpreted as irrigated strips and large oases in the midst of a desert of ochre-colored sand.

The small amount of water on the planet is mostly precipitated in the form of snow or hoar frost about the poles during the respective winters of each hemisphere, from which the greater part of it melts away with the succeeding spring and summer. As the snow melts it forms an annular sheet of water at the base of the polar cap, and this water is conveyed by means of canals traversing the irrigated strips to all parts of the planet. All this is accomplished by the psychic life of Mars, and proves beyond a peradventure that on its surface there dwells a race of beings of great industry and high intelligence. This is to all appearances the same race that was developed at the beginning of the Psychozoic age and which corresponds to the human race upon the earth. It constitutes the characteristic life of that age which now assumes great geologic importance and constitutes the leading feature of Arean geology, or areology. All this is clearly brought out by the following table and by the Martian dial which follows, and which may now be directly compared with those of the earth.

Geologic Periods	*Years*	*Hrs.*	*Min.*	
Archean	18,000,000	4	30	
Algonkian	18,000,000	4	30	
Cambrian	6,000,000	1	30	
Silurian	6,000,000	1	30	
Devonian	6,000,000	1	30	
Carboniferous	6,000,000	1	30	
Triassic	3,000,000		45	
Jurassic	3,000,000		45	
Cretaceous	3,000,000		45	
Tertiary	3,000,000		45	
Psychozoic	24,000,000	6		
Total age of Mars	96,000,000	24		
Probable future remnant of Psychozoic time on Mars	1,000,000		15	

As the parallelism is supposed to be complete to and some distance into the Psychozoic period, it is that period only which challenges our attention. It is its bare simplicity that excites our awe. Can it be possible that an intelligent being has inhabited that world during a period as great as that which separates us from the Silurian? This would seem to be contrary to the whole course of the earth's geologic history. All the more prominent families of animals and plants that have lived on the earth since the beginning of Paleozoic time have been restricted to the ages in which they were developed. The Cambrian trilobites, the Silurian molluscs, the Devonian fishes, the Carboniferous insects, the Jurassic reptiles, the Cretaceous birds, and the Tertiary mammals, have for the most part belonged to families that disappeared at or near the close of their respective geologic epochs. The same is true of the great vegetable races. Only a few unspecialized forms of either animals or plants, such as Lingula and Equisetum, have persisted through two or several geologic periods. How could a highly specialized being, such as the psychic occupant of Mars must be, have persisted unchanged through so immense a lapse of time?

We can only answer that we do not know, that perhaps that race itself has been transformed and re-transformed and no more resembles its Quaternary ancestor than the horse resembles the Eohippus. But certain it is, and the remark is now trite, that on this earth the psychic animal man has all but ceased to feel the transforming influence of his environment, and that, instead, it is he who has begun, and on a grand scale, to transform his environment. True, he has not probably as yet so altered the visible aspect of the planet he inhabits that the inhabitants of Mars could, even with such appliances as we possess, see his work and recognize it as that of a rational being. This the Martians have done for us, and it marks a vastly higher social efficiency on their part than man has attained. But the question will obtrude: Given the origin of a rational being upon a planet, what reason exists why it should not continue to remain there as long as the conditions of existence permit? Those who insist that something else will terminate its existence are bound to point out what that something is.



DIAL OF THE COSMIC DAY OR LIFE HISTORY OF MARS

What, then, is the lesson of Mars? It has a twofold form, but it is one lesson.

In the first place Mars teaches us in the most unmistakable manner that the earth must follow in its footsteps; that its mountains must be worn down and its seas filled up until it shall become a smooth ball; that its waters and its atmosphere must be absorbed and become incorporated in its solid crust, though in this respect it will remain long at the stage at which we find Mars to-day, its final death following that of Mars by the same time-distance as did its birth. In the above table I have assigned to Mars one more million years, which any one is at liberty to double or to halve. On Mars we can, as it were, see with our own eyes a race of vast antiquity and supreme wisdom, clinging desperately to the orb that bore it, half gasping for breath, and hoarding every drop of its precious water, but doomed in the relatively near future to face the lingering death of a dying world. This is indeed sad, and it is perhaps still sadder to reflect that such is the fate of all life including that of our own globe and our own race, and that the day must come which shall, to change two letters of a celebrated line of Corneille,

Voir le dernier hu*main à son dernier soupir.*

But there is a reverse to this sombre medal. The human race is supposed to have existed between 200,000 and 300,000 years, let us say one quarter of one million years. It has been conscious of its existence only about 10,000 years, and really alive as a psychic being less than 5,000 years. The most that it has accomplished of any value to itself has been done within 2,000 years, and its great work within 200 years. In a word, relatively speaking, man has only just begun to exist. His golden age, as Saint-Simon said, is before him and not behind him. His history is but the threshold of the Psychozoic age. The whole of that immense period lies before him. The conditions of existence on this earth are now at their optimum. Abundance of air and water, heat and light, great variety of surface, soil, climate, mineral resources, and all the materials and forces of nature ready to yield to the magic wand of science. There are no indications that these conditions will change in an entire geologic epoch. These favorable conditions are certainly liable to last as long as the Tertiary period just closed has lasted, viz., 3,000,000 years. They may continue during the first half of the Psychozoic period of Mars, or 12,000,000 years. And what does a million years mean? Glance at the two dials and note the human period. Contrast it with any full geologic epoch and reflect upon its significance! For us the Psychozoic age, or any considerable part of it, means *eternity*. Thus viewed, man's prospects, instead of being dark, are fairly roseate, and the contrast with that old decadent orb that is now telling us its story, instead of depressing us, should inspire us with thankfulness that we are young, with faith in an unlimited future, and with buoyant aspirations for the progress of humanity.

March 16, 1907—Ætat. 65.

530. The Establishment of Sociology*

History.—Written October 27, to November 29, 1906. The rough draft was written October 27, to November 2, and was left untouched until November 27. It was then revised and the final draft was written on the two following days. The address was delivered December 27. I sent the manuscript to the Secretary of the Society on January 2, 1907, who attended to its publication in accordance with an arrangement between the Society and Dr. Small, Editor of the *American Journal of Sociology.* Proof was received on February 7, and the number containing it reached me on March 18. Reprints arrived on March 30. Vol. I of the Publications did not reach me till June 8.

A brief account of my election as the first president of the American Sociological Society may not be out of place here, especially as there seems to have been some misunderstanding with regard to it. A full account of the "Organization of the American Sociological Society" was prepared by the Secretary, Dr. C. W. A. Veditz, and published in the *American Journal of Sociology* for January, 1906, and I need not repeat what is there said. It is only my personal connection with the matter that I wish to explain. The fact that at that time, Dr. Veditz was located in Washington led some to suppose that the movement was inspired by me, and that he and I were working together to bring about the result. As a matter of fact, I had favored the founding of such an organization for many years, and approved of Mr. L. T. Chamberlain's proposal as far back as 1895 to organize an "Institute of Sociology." When Dr. Veditz proposed to move in the matter I of course encouraged him and willingly signed the original call for the Baltimore meeting, but I had absolutely nothing to do with the arrangements. The credit for these and for the entire initiative, as well as for the successful completion of the scheme, is wholly due to Dr. Veditz. I was too busy to attend the meeting of the American Economic Association, and could only spare time to run over from Washington to each of the meetings of the sociologists, returning the same night. I dined with Dr. Veditz after the meeting on December 27, and we had a good talk, but, so far as I remember, it was a general rejoicing over the adoption of my motion to make the Society independent and not a mere section of the Economic Association, and not one word was said about officers. In fact, I had considered that unnecessary, as Professor Giddings was the leading figure in the meetings, and I supposed of course he would be the first president and was prepared to support him and vote for him. And when, the next night the committee on officers presented my name, I never was so much taken aback in my life, as I tried to say in my stammering speech after being led to the chair. Professor Giddings' remarks were certainly noble and delightful, and I take this my first opportunity publicly to acknowledge them and to thank him for making them.

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* Address of the president of the American Sociological Society at its first annual meeting in Providence, R. I., December 27, 1906.

This address was reprinted entire in the *Plebs Magazine*, running through two numbers.

The American Journal of Sociology, Chicago, Vol. XII, No. 5, March, 1907, pp. 581–587; Publications of the American Sociological Society, Papers and Proceedings First Annual Meeting, Providence, R. I., December 27–29, 1906, Vol. I, Chicago, 1907, pp. 3–9; The Plebs Magazine, Oxford, Vol. II, No. 2, March, 1910, pp. 36–39; No. 3, April, 1910, pp. 53–55.

I DO not propose on this occasion to enter into any defense of the claims of sociology to be called a science. I wish simply to show that its history, and the steps in its establishment, do not essentially differ from those of other sciences.

On a former, somewhat similar, occasion I took the same position, and as the words then spoken in a foreign tongue have never been reproduced in our own, they seem to form a fitting introduction to this address. This is what I said:

> Certainly no member of the International Institute of Sociology doubts that there is a sociological science, but certain persons suppose that there is a difference between this and other sciences. The fact that the foundations of

the science are still being discussed, and that sociologists differ with regard to them, while the foundations of other sciences seem to be recognized by all, causes it to be imagined that sociology is a science different from the rest. But one needs only to study the history of other sciences to see that such is not the case. Without entering deeply into this study, it is sufficient to consider the most completely established sciences at a special epoch in their history. Everyone knows that astronomy is the most exact and the most perfectly established science that we have. Let us consider it, for example, in the seventeenth century. Descartes was acquainted with the theories of the ancients. He knew the Ptolemaic theory. That of Copernicus was familiar to him, as well as the modification of that theory proposed by Tycho Brahe. Modern astronomy is chiefly based on the theory of Copernicus, and its exactness depends entirely upon the law formulated by him of the revolutions of the planets. But was astronomical science established at that period? Certainly not. In the possession of all this knowledge the greatest genius of the seventeenth century rejected the true principle and elaborated a new hypothesis almost entirely different from all that had preceded it—a massive and complicated hypothesis which the modern world has almost entirely forgotten. Astronomy in the seventeenth century was, then, in a condition somewhat similar to that of sociology to-day.

It would be easy to show that the same was true of physics before the discovery of the law of gravitation, and also that it was true of chemistry before the discovery of the true nature of combustion. As regards chemistry, it is appropriate to mention it in this amphitheater devoted to its study,* and it is the glory of France and of the immortal Lavoisier to have made that great discovery which lifted chemistry out of the state of vague theories and false hypotheses, and placed it on the firm and secure basis on which it stands today.

But there is a difference between the modern theories of sociology and the theories which prevailed in the other sciences before their final establishment. The theories of Ptolemy and Descartes in astronomy were false, or they contained only a minute germ of truth. The theory of phlogiston in chemistry was almost entirely false. This is not the case with modern theories in sociology. The organicist theory is not false, nor is that of imitation, nor that of the struggle of races, nor that of social control, nor yet that of the consciousness of kind (these last two come from America, and I do not speak of principles laid down by myself). These hypotheses, and almost all others in sociology are true, or contain a considerable part of the grand sociological truth which is the final synthesis of them all.†

More recently a South American, Ernesto Quesada, professor of sociology in the University of Buenos Ayres, has uttered very similar words, going, however, much more fully into the subject.‡ He was practically driven to this course by a remark of the retiring dean, Miguel Cané, of the university, in a public address, reflecting severely upon the study of sociology. He said, among other things, that "sociology, far from being a science, was little more than empty verbiage," and added that

he would see with great satisfaction the abandonment of a word more pretentious than expressive of anything real, and more capricious than scientific.

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* The congress held its session in Hall of Chemistry of the Sorbonne.

† Annales de l'Institut international de Sociologie, Vol. X (Paris, 1904), pp. 50–52.

‡ Ernesto Quesada, "La sociología: Carácter cientifico de su enseñanza," *Revista de la Universidad de Buenos Aires*, 1905, Vol. III. (Reprint, Buenos Aires, 1905, 43 pp., 8vo.)

> To study the various human groups, the causes that actuate them, and all the other determining elements of their respective activities, is to set forth principles of a general character, which, though accepted only provisionally, serve as a basis for further investigations. But from this to the erecting into a science, with fixed, immutable boundaries, of a mixture of hypotheses and empirical assertions, and calling it a science in the same sense as algebra or mechanics, seems to me an enormous stride. A science ought to be that impregnable region where alone reign truths and proved laws. If twenty professors, all working along the lines of the modern sociologists, were charged with the preparation of a program of the subject, I am certain that they would present twenty different programs, each conforming to the quality of mind, personal education, and peculiar method of the author; whereas, of twenty professors of geometry there would not be one who would dare to attack the hypotheneuse and attribute to it properties that it does not possess.

Professor Quesada replied to all this very fully and with great ability, but he failed to point out the complete irrelevancy of Dean Cané's attack, comparing sociology to mathematics, which is not a science of concrete things at all, but simply the norm by which all science is tested, and even referring to algebra, which is only an instrument, or tool. to be used in the solution of problems of quantity.

But Professor Quesada shows very clearly that no science is absolutely fixed. All are compelled to start with certain postulates—*i. e.*, unproved propositions, or assumptions—and build upon those; and he enumerates the chief of them as defined by the masters in each science. He shows, moreover, that these postulates are often doubtful, and that several of them—as, for example, that of the atom of chemistry—are undergoing profound modification with the advance of our knowledge. He may be said to have made out a clear case that there is no "impregnable region where alone reign truths and proved laws," and that all the sciences are perpetually *in fieri*, in the same sense as is sociology. He does much more; for he proceeds to show not only that sociology is already a science of great importance, but that it may be applied directly to practical affairs; and he promises in his lectures to show the legislators and statesmen of Argentina how they may utilize it in advancing the interests of their own country and people.

All the attacks upon our science might easily be met in a similar way, and I have taken some pains to collect all the objections I could find and to ascertain the fallacy that underlies each one. I had thought of presenting the result of this study; but not only would it require more time than can be devoted to it in this address, but, upon mature consideration, I conclude that it is not worth the while, as sociology is marching over all these stumbling-blocks, and nothing that its enemies can do will greatly check its sure and steady advance. What I propose to do, therefore, is simply to draw your attention to a few of the steps that sociology has taken, and endeavor to point out what has actually been done in the direction of its establishment as one of the great sciences.

Probably the most important result that sociology has accomplished is that of showing what society is; that, if it is not an "organism"—and few now would go that far—it is at least a great organization, bound together by organic ties in all its parts. To be more specific, sociology shows us that human institutions constitute the structures, organs, and organic parts of society, and that they are not independent, but are connected into one great system, which is society. It has not only done this as the result of a study of society in its finished form, but it has confirmed this truth by a study of the origin of human institutions. It has shown how they have arisen. It has traced them back to their primordial, undifferentiated forms, and studied their development from this state of homogeneity to their present state of heterogeneity. It has watched first their differentiation and then their integration.

The general result is that we have come to know what society really is. Sociology has enabled us to orient ourselves in this great maze of human life, to see what the human race is, how it came into existence, approximately when and where it began, in what ways it has developed and advanced, and how it has come to be what we find it. "Know thyself," said the old Greek philosophers; but man never did really know himself until these studies of origins had been undertaken and successfully carried out.

Involved in this we have the true genesis of all the most important human institutions—religion, language, marriage, custom, war, cannibalism, slavery, caste, law, jurisprudence, government, the state, property, industry, art, and science. Instead of a great bewildering maze, a vast meaningless chaos, society reveals itself as a true genetic product of uniform laws and forces, a product of social causation, and stands out in clear relief against the background of history.

But sociology has done more than this. It has not only discovered the laws of society; it has discovered the principles according to which social operations take place. It has gone farther even than physics, which has thus far only discovered the law of gravitation, but has not yet discovered its cause or principle. Sociology has not only established the law of social evolution, but it has found the principle underlying and explaining that law. Just as in biology the world was never satisfied with the law of organic evolution worked out by Goethe and Lamarck until the principle of natural selection was discovered which explained the workings of that law, so in sociology it was not enough to formulate the law of social evolution, however clear it may have been, and the next step has been taken in bringing to light the sociological homologue of natural selection which explains the process of social evolution. That principle is not the same as natural selection, but it serves the same purpose. It also resembles the latter in growing out of the life-struggle and in being a consequence of it; but, instead of consisting in the hereditary selection of the successful elements of that struggle, it consists in the ultimate union of the opposing elements and their combination and assimilation. Successively higher and higher social structures are thus created by a process of natural synthesis, and society evolves from stage to stage. The struggling groups infuse into each other the most vigorous qualities of each, cross all the hereditary strains, double their social efficiency at each cross, and place each new product on a higher plane of existence. It is the cross-fertilization of cultures.

The place of sociology among the sciences has been definitely fixed. It stands at the summit of the scale of great sciences arranged in the ascending order of speciality and complexity according to the law of evolutionary progress. It rests directly upon psychology, in which it has its roots, although it presents a great number of striking parallels with biology, chemistry, physics, and even astronomy, showing that there are universal laws operating in every domain of nature. The motor principle of sociology is psychic, and the study of this principle has shown that social phenomena are produced by the action of true natural forces, which, when abstraction is made of all perturbing elements, are found to be as regular and reliable as are the forces of gravitation, chemical affinity, or organic growth.

As a result of this it has been possible to establish the subscience of social mechanics and to work it out with something like the completeness that has been attained in the mechanics of physical nature. At least it has been possible to distinguish clearly between static and dynamic phenomena in society. This distinction, dimly seen by Comte, and still more dimly by Spencer, when fully and clearly apprehended, throws a flood of light over the whole field of social phenomena. Social statics is found to constitute the domain of social construction, and to explain the origin of all social structures and human institutions. It underlies the social order. Social dynamics, on the other hand, is the domain of social transformation, and explains all change in social structures and human institutions. It is the science of social progress. The laws of both these sciences have been to a large extent discovered and formulated, and their workings described.

All this has been accomplished by a careful study of the social energy alone. But sociology has not stopped here. It has plunged boldly into the far more difficult and recondite field of social control. The social energy is so powerful as to exceed its proper bounds and threaten the overthrow of the social order, and would do so but for some effective curb to its action. The motor power of society has to be guided into channels through which it can flow in harmony with the safety of society. This guidance has been furnished by the higher mind or intellect of man. This guiding or directing agent is a far more subtle element than the motor force itself, and one much more difficult to understand. But sociology has not shrunk from the task of studying it and unfolding its laws and operations, and these have been sufficiently mastered to be in large part formulated and described. This fairly complete mastery of the dynamic and directive agents of society has placed sociology in position to deal in a thoroughly scientific way with all the facts and phenomena of society—with its origin, its history, and its present condition.

Finally, with the light shed by social dynamics on the spontaneous modification of social structures and the consequent progress of society in the past, and further guided by the established law of social uniformitarianism, which enables us to judge the future by the past, sociology has now begun not only in some degree to forecast the future of society, but to venture suggestions at least as to how the established principles of the science may be applied to the future advantageous modification of existing social structures. In other words, sociology, established as a pure science, is now entering upon its applied stage, which is the great practical object for which it exists.

March 16, 1907—Ætat. 65.

531. The Fine Arts as a Dynamic Factor in Society

History.—Remarks on the paper with the above title read December 28, 1906, by Mrs. Johanna Odenwald Unger. Written out February 16–17, 1907. In seeking for suitable persons to discuss Mrs. Unger's paper, I called on Professor Wm. C. Poland, Professor of the History of Art in Brown University, and Director of the Museum of Fine Arts, and asked him if he would be willing to take part, explaining the nature of her paper, and later seeing that a copy of it was placed in his hands. The morning that the paper was to be read I received a letter from him stating that he was ill, could scarcely speak, and would not be able to attend. Dr. Small presided over that meeting, and when Professor Poland was called upon I rose and read the letter. I then made these remarks entirely without preparation. Dr. Small had engaged a stenographer to report the off-hand speeches, but on February 16, I received a letter from Dr. Veditz saying that the stenographer had not sent him any reports, and that I had better write my remarks out, which I immediately proceeded to do, and sent them to Dr. Small on the 17th. On February 21, I received a letter from him saying that the stenographer's report had already gone to press. No proof was sent me, and the report as it appeared is in pretty bad shape. The reader, however, can imagine from it what I said. I have, however, the rough draft of what I wrote out on February 16, and though it may differ somewhat in the wording from my remarks, it is substantially the same, and I introduce it here as a sort of corrective of that.

I am sure we all deeply regret that Professor Poland is unable to come. It is far from me to pretend to offer a substitute for his remarks, but a single thought has occurred to me, which, with the permission of the Society, I will take this opportunity to express as well as I can without having attempted to formulate it. I feel keenly the force of Mrs. Odenwald Unger's criticisms of the passages quoted by her from my books, and so far from making any attempt to defend myself, I acknowledge myself a convert to her view, and what I shall say will be wholly on her side of the question.

There is an important sociological law, for the most part thus far overlooked, which seems to me to lie at the foundation of Mrs. Odenwald Unger's contention. This is the law that very many forms of culture begin as ends and end as means. A large class of these are at first in the nature of fine arts and are gradually transformed into useful arts. All of them begin by appealing to the esthetic sense and ultimately come to appeal to the moral sense.

Without pretending to enumerate them all we may first consider clothing, which, as all ethnologists now know, began as ornamentation, for which paint and tattooing were the frequent substitutes, and was only gradually converted into a useful art. Architecture is another conspicuous example, and it is still a question whether its esthetic or its practical aspect is the more prominent, but the latter is certainly gaining upon the former.

Passing over many other arts that might be specified, we come to language. The more language is studied the clearer it becomes that speech began as the utterance of more or less musical sounds with rhythmic cadences, often accompanied by bodily movements constituting the primitive dance, and thus both language and music are of terpsichorean origin. Many lower races, and some uncultivated people among civilized men still often speak in a sort of cadence or fall into the manner that is called the recitative. When the stage of written language was reached, it is now well known that its first form was rhythmic and esthetic, to such an extent that even laws and edicts were expressed in measure. Thus poetry preceded prose, and all the ancient sacred books are poems, while early histories are all epics.

The most striking example of this law is found in that more modern form of written language which goes by the name of literature. Literature down to the end of the eighteenth century was simply a fine art. Its sole object was to please by a display of elegant diction. It is now almost wholly transformed into a vehicle for the conveyance of thought. It began as an end in itself and it has become a means to higher ends. The value of a good style to-day consists entirely in its power better to express ideas and advance knowledge. From an esthetic factor literature has become a dynamic factor. The great literary productions of the last century, and especially of the last half century, are those that have moved to action and wrought social reforms. Did not Dickens abolish the Fleet and Victor Hugo the galleys?

Need I go farther and add to the list the drama, which began as an amusement and has now become an engine of moral reform? The painters that are now adjudged great are those who can paint a "Man with the Hoe," and arouse sympathy for benighted humanity, or those who can picture political oppression or social degradation and sow the seeds of revolution. Even sculpture and music, as Mrs. Odenwald Unger has shown, are passing from the condition of esthetic ends to that of ethical means.

Viewed in this light, which is certainly a true light, it is obvious that the fine arts not only may become, but in a great degree have already become, a dynamic factor in society.

Ibid., pp. 667–668 [89–90].

March 16, 1907—Ætat. 65.

532. Social Darwinism

History.—Remarks on the paper with this title read December 29, 1906, by Professor D. Callin Wells. Written out January 2, 1907. My experience in Europe that same year (see infra, Nos. 533, 535) had made me somewhat familiar with the phrase "social Darwinism," and this alone led me to consent to discuss a paper by that title, and I was announced to open the discussion. But when, on December 20, I received an abstract of Professor Wells's paper, I saw that he meant quite a different thing by the phrase from that of Loria and Novicow. Still, there was enough to be said on it, and, although I had not had time to write out my remarks, I was fairly well prepared. When on January 2, 1907, Dr. Veditz asked me for my remarks for publication I wrote them out and sent them to him. I made a rough and a final draft and of course sent the latter. What I received back as copy accompanying the proof, which reached me on February 11, was not my manuscript but a typewritten copy of it, an apparently superfluous labor.

Ibid., pp. 709–710 [131–132].

May, 1907—Ætat. 65.

533. Les Luttes Sociales et les Luttes Biologiques

History.—Written March 30, to June 6, 1906. On May 11, 1905, I received the announcement of the Sixth Congress of the International Institute of Sociology to meet in London in July, 1906, and to have for its subject *Social Struggles* (*Les Luttes Sociales*). On the 16th I wrote to Dr. Worms that I would attend and would offer two papers, naming as one of them *Le Darwinisme Social.* On June 28, I received from M. Jacques Novicow his new book: *La Justice et l'Expansion de la Vie.* I did not for some reason begin reading it till August 12. In glancing it through I noticed that Book II (pp. 251–367) was entitled *Le Darwinisme Social.* As that was the title of the paper I had proposed to read at the Congress, I read that part first. I found it to be almost entirely an attack on me and the views I had set forth in Chapter X of *Pure Sociology.* He seemed to know nothing of Gumplowicz and Ratzenhofer except through that chapter. His treatment of the subject was so superficial, emotional, and unscientific that I did not regard it as worth my attention. He knew nothing of Darwin or Darwinism. It was evident, however, that if I read a paper on that subject and with that title after the appearance of his book in which my name was so freely used, it would be considered a reply to his attack. This I was anxious to prevent, and on August 21, I wrote to Dr. Worms to withdraw that title, as I should not present such a paper.

The matter rested there until March 26, 1906. Knowing nothing of the program, but presuming that Novicow would introduce the subject at the London meeting, I thought I would have some remarks ready and perhaps take part in the discussion.

Changing the title to the present one, I commenced on that day to write out some such remarks and I finished the rough English draft on the 30th (see infra, No. 535). The same day I commenced the French draft which I finished on April 3. I sailed for Europe on April 21, and went direct to Paris. I could not therefore submit my French paper to the professor in Washington, and on May 10, I found a competent person in Paris to do this work. It was Mlle. Fanny Borgès, 25, Rue Panthièvres, whom I am glad to mention, and who did all of that class of work that I had to do that summer. She revised the paper and we had a number of sessions over it. On May 20, I commenced embodying her suggestions in a final draft which was finished on May 23, and submitted again to Mlle. Borgès. The last finishing strokes were given to it on June 6.

I attended the London Congress which met on July 3, but no papers were read on that day. I had two papers, and had taken the precaution to have them with me in both French and English, as it was not decided till I arrived there in which language papers should be presented. The decision was complimentary to the English, as it was based on the fact that while the Continental members could not generally understand English, the English members could understand French. A program had been prepared by Dr. J. W. Slaughter and I received it in Paris. Dr. Worms had insisted that my paper be treated as a principal one and not as part of the discussion. The morning session of July 1, was to be devoted to papers setting forth the *Caractères généraux des luttes sociales*, in which Novicow had the first and I the second paper. I could not therefore judge from the program just how he would treat the subject, but I presumed he would go over the same ground as in his book. He must also have been ignorant of the nature of my paper. The real title of his paper, as it appears in the *Annales*, was *Critique du Darwinisme Social*, and he almost began it with a quotation from *Pure Sociology*, and continued to use my name throughout and to present the same shallow, sentimental notions that he had expressed in his book. As a considerable part of my paper was directed against these notions (I used no name) it was also an answer to his paper, and if we had sought to combat each other we could not have adjusted our papers more perfectly to that end. After reading his paper he was asked to take the chair and preside over the meeting. As I spoke from the platform, I stood close by him, but did not see his face while reading my paper. Dr. Van Eeden, who was, like myself, a guest of Lady Welby at Harrow, and who was in the audience, amused us all at dinner by describing the contortions of Novicow's face as I scored point after point against him. I handed my paper to Dr. Worms before the Congress adjourned, and the volume containing it was received on June 6, 1907. Although no proof was sent me, I have never discovered any typographical errors in it.

Annales de l'Institut International de Sociologie, Tome XI, Paris, 1907, pp. 111–126.

May, 1907—Ætat. 65.

534. La Sociologie des Partis Politiques

History.—Written March 14, to June 5, 1906. The circumstances connected with the Sixth Congress of the International Institute of Sociology were stated in the history of the last paper (supra, No. 533). That paper was not my principal one, and was, in fact, a kind of afterthought. This was the paper which I proposed to present to that Congress, and I sent the title to Dr. Worms on May 16, 1905, with the other, but mentioning it first in order. It seemed to me that political struggles were also social struggles. The rough English draft, as a basis for the French paper, was written from January 31, to March 13, 1906, from which latter date I began at once to write the French outline. It was practically completed on March 25, but I continued to tinker it till near the time I left for Europe (April 21). I had not been able to find in America a French translation of Herbert Spencer's works from which I wished to quote quite extensively, and immediately after my arrival in Paris I made it my first business to hunt these translations up. After considerable search I found them in the library of the Institut, and on May 3, I copied them all out of the books. Later I bought of Alcan, who published it, the 7th edition of *L'Individu contre l'Etat* (*The Man versus the State*) just out (1906), and brought it back with me. The principal passages that I used are in that. This paper was submitted to Mlle. Borgès along with the other and she revised and corrected both. But the other was given precedence as I then knew it would be the first to be read. She did it in instalments, and brought me about half on May 29. I then commenced writing the final draft and she kept me supplied. I finished it on June 5, but submitted it to her again, and the last touches were not put on till the 14th. It was read before the

Congress on July 6. Lord Avebury came in after I began. He sat on the platform near me and after I finished he opened the discussion. His remarks are not reported. I failed to get reprints of either of my London papers, not ordering them in time, and no proof was sent me.

Ibid., pp. 393–417.

December 5, 1907—Ætat. 66.

535. Social and Biological Struggles*

History.—Rough outline written March 26, to 30, 1906. Final draft written May 24, to 26, 1906. After the French draft had been completed, not being certain that I should not have to read it in English, I decided to make a neat copy in a large round hand that I could use to read from if necessary. As soon as the volume of the *Annales* containing my French papers arrived, viz., on June 6, 1907, I wrote to Dr. Small to know whether he would like to publish them in English, as this would now be permitted by the statutes of the Institute. He replied that he would like them, but could not print either till the November number. I intended to hold them awhile, but I soon received another letter saying that he would like to put them in type at once. As they were entirely ready I sent them the same day I got this letter, which was June 15. On July 1, I received the proof of this one, but it had to wait till the November number, which did not reach me till December 7. My reprints arrived on January 21, 1908.

The American Journal of Sociology, Chicago, Vol. XIII, No. 3, November, 1907, pp. 289–299.

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* From the *Annales de l'Institut International de Sociologie*, Tome XI, pp, 111–126, Paris, 1907.

December 5, 1907—Ætat. 66.

536. Relations of the Social Sciences.—A Symposium

History.—Written October 8, 1907. On October 7, 1907, I received the following letter:

October 5, 1907.

PROFESSOR LESTER F. WARD:
DEAR SIR:

I am sending under another cover reprints of four papers which have led to the following request:

Will you contribute 400 to 500 words to a symposium in the American Journal of Sociology on the series of questions submitted herewith? The one group of questions was drawn up by Dr. Hoxie, the other by myself. If it is impracticable to answer the whole series, will you reply to one or more of the questions which may seem to you most pertinent? Our desire is not to elicit expressions of opinion about the merits of the printed discussions, but about the main questions concerned. I may add that we have no intention of inflicting upon you the burden of reading the four papers, but they are sent rather as evidence that the questions proposed have taken shape in the course of serious debate.

This request will be sent to twenty representatives of the social sciences.

The contributions should be in hand not later than October 25th.

As the subject is important to every student of the social sciences, it is hoped that you will be able to join in the discussion.

With thanks in advance for your favor,
Sincerely,
ALBION W. SMALL.

I

1. Do you believe that reality is fixed in its ultimate nature and constitution? If not, (1) Can you assign any absolute or ultimate or inviolable constitution to it; (2) Is any science or any group of correlated sciences capable of giving any "normal" or exclusively valid or ultimate or completed view and explanation of reality?

2. Is the following a valid definition of a social science: "A social science is an examination and interpretation of human experience as such from some dis-

tinctive human standpoint, aspect or interest; or it is an attempt to describe and explain or interpret human experience as it is ranged about and related to some one special interest which is for the time being regarded as the end of human experience." If such a definition be accepted, (a) Is any particular social science standing by itself merely or predominantly analytical in its treatment of human experience; (b) Does not a science thus constituted make constant use of the results and conclusions of other similarly constituted sciences; (c) Can sciences thus constituted then rightly be called fragmentary; (d) Can they rightly be called independent; (e) How can such sciences be "correlated" so as to give a complete view of human relations?

II

3. What is your opinion of the proposition: The indicated function of the social sciences at their present stage is to cooperate in finding a detailed content for some comprehensive conception of human relations.

4. What is your opinion of the following as a formula of such guiding conception?—For our intelligence the most central process within the range of experience is the evolution of human personality; for our intelligence, therefore, every separable phase of human experience must get its meaning and valuation from the connections which we discover between it and the central process of the evolution of persons. (4th reprint, p. 219.)

As my part of the symposium I made the reply given below.

Ibid., pp. 397–398.

I will try to answer the points *seriatim*, but I do not think anything I can say will be worth anything, certainly not worth publishing. I read all the articles as they appeared.

I

1. I do not understand "reality" to be anything in and of itself, or to have any "nature" or "constitution." That would belong to the old ontological metaphysics. Anything that is real, *i. e.*, that exists, is *a* reality, and there are as many realities as there are things that exist. When you say "reality" I understand you to mean real things, nearly in the sense of what is true, but regarded objectively—nature, matter, and its relations, "things as they are."

2. Both the definitions of a "social science" seem correct, but a dozen others might also be given and be correct. I have always confined it to "human" phenomena, and given my reasons. Some extend it to animals.

(*a*) Some treatises on, or treatments of, social science are predominantly analytical and others are predominantly synthetic; *e. g.*, my *Applied Sociology*, Part II, is an analytical treatment, and my *Dynamic Sociology*, Vol. II, is a synthetic treatment of the same subject. (*b*) It seems to me that both analytical and synthetic treatments of almost any science, and especially of social science, must "make constant use of the results and conclusions of other similarly constituted sciences," and of others that are not "similarly constituted." (*c*) The term "fragmentary" does not seem to be a happy one by which to characterize any of the established and recognized sciences, not even the so-called special social sciences. (*d*) They may properly be called "independent" in the sense that they are complete disciplines, but no science is in reality independent. The interdependence of the sciences is admitted by all. (*e*) There are two ways in which the sciences may be "correlated." The great primary ones can be arranged in serial order as determined by their relative generality and speciality, and their relative simplicity and complexity (these always going hand in hand), as well as by the natural filiation, or the affiliation of the higher upon the lower. The lesser secondary sciences should be arranged in synoptical form under the primary ones, as geology under astronomy, zoölogy under biology, etc. All the "special social sciences" thus fall under sociology.

II

3. The proposition seems to me a little obscure. Perhaps this is due to redundant words. If it means that the special social sciences co-operate in the creation of a general social science, there can be no objection to it. It follows from the synoptical arrangement referred to under I, 2 (*e*).

4. The use of the words "personality" and "persons" obscures the meaning, and I am not sure what that is. The evolution of individual men seems to be involved in the evolution of the human race. This belongs rather to anthropology than to sociology, but is a condition to the evolution of society. "The most central process" for the sociologist is the formation of groups, or associated "persons." (I may entirely misunderstand this number.)

December 22, 1907—Ætat. 66.

537. What Brings Out Genius? Is it Inheritance, Opportunity, Training, Environment, or Luck

History.—Written June 7, to July 18, 1907. On May 31, 1907, I received a letter from Mr. W. B. Parker, in which he says:

> "I write to invite you to prepare an article for the Associated Sunday Magazines, which, as perhaps you are aware, are distributed through great metropolitan papers so as to reach a very large proportion of the reading public of the United States.
>
> "The article which I invite from you would be on *How Genius Occurs* and would be in some measure an attempt to account for the rise of genius and its distribution.
>
> "I am led to propose the matter to you by a conversation I had some time ago with Justice Holmes of the Supreme Court, whom I found, when I called upon him, reading your *Outlines of Sociology* [probably *Applied Sociology*] and very deeply interested in some of the propositions there laid down. As we talked about genius and its occurrence, I formed in my mind the scheme of a highly popular expository article illuminated with familiar examples. I shall be glad if you will consider writing this, taking up the cases of Lincoln, Grant, Edison, and others who more or less readily suggest themselves, and in words of one syllable, as it were, expounding the matter of the origin of genius."

There was another paragraph which I need not quote, and the letter concluded by saying that the article

> "need not be more than three thousand words in length, and for which we should propose to pay the sum of $150.00."

I answered the letter the same day offering to undertake it if allowed to do it scientifically, and not compelled to indulge in popular claptrap about self-made men. He replied on June 5, with some quite lengthy suggestions, cautioning against too scientific language, but thinking that I could probably make it satisfactory. On June 7, I commenced a rough pencil draft of the article. I had written to Parker the day before referring him to some of my articles in the *Forum* and sending him a reprint of one of them, and on the next day I received a letter in which he intimated that the article must be more popular than the ones in the *Forum*, and saying:

> "For our purpose an article cannot be too anecdotal, picturesque, terse, concise, and graphic."

Confident that I could not comply with all these requirements, and certain that I would not stoop to the attempt, price or no price, I wrote him to that effect, and virtually declined to go further with the matter. I also stopped writing on the article. On the 18th I listened to an interesting lecture by Professor Bliss Perry, editor of the *Atlantic Monthly*, on *The Academic Temper*. It caused me to conceive of the possibility of getting the article printed in the *Atlantic*, and I concluded to finish it and send it there. The next day I finished the rough outline, which, though written in pencil, was after all about as I wanted it. Another day and I had put it in final form, and I sent it to Professor Perry with a letter. I supposed I was through with Parker, but on the 27th I received a letter from him saying he might call on me soon. He did call on July 1, and we spent several hours together. I had the pencil draft, and he said he would like to read it. I let him do so, and he said that if the matter rested entirely with him he would not hesitate to accept it, but that it had to go before a board. He asked me to let him take it with him and lay it before the board, and he carried it off. I explained to him that I was sure it was not what he wanted, and had sent the final draft to the *Atlantic*, but I told him it was very doubtful whether it would be wanted there, and that if the board approved of my article I would recall it and send it to them. I was saved the trouble of doing this by the return of the article on July 4, with a polite note from the editors saying that they must give precedence to other subjects. I sent it to Mr. Parker on the 8th. On the 17th I received a proof of the article and a letter from Parker, asking me to make some additions to it, and saying:

> "In the body of the article we should be glad if you could make an allusion to some American and modern examples of genius, or, if you find it impracticable to do that in the body of the article, we should be glad if you would add a paragraph or two at the end, to point the moral, as it were."

What I did do was to insert the names in parentheses of the Greek masters, and to add the last five paragraphs. On the 24th I received a final proof of that article as amended and a letter stating that it was now satisfactory, and formally accepting it. On August 3, I received the check in payment ($150.00). As I did not devote more than ten hours to it all told, it was the most remunerative piece of work I ever did. They took their time in printing it, and reserved it for their Christmas number. It appeared simultaneously on December 22, 1907, in nine leading journals representing as many of the principal cities of the United States, to wit: *The Boston Post, The Pittsburg Post, The Washington Star, The New York Tribune, The St. Louis Republic, The Philadelphia Press, The Minneapolis Journal, The Chicago Record-Herald*, and *The Rocky Mountain News*, and *Denver Times*. I obtained copies from most of these.

The reader of this article has of course perceived that it is only the quintessence, as it were, of my *Applied Sociology*, as set forth in Chapters IX and X of that work. To show that this is a serious and practical matter and not a mere speculation I will draw attention to one aspect of it and quote the most competent judge that probably could be named. On page 266 of that work I took occasion to criticize the "exceptional man" theory, and quoted the views of Professor Huxley and Mr. Carnegie on that point, which I regarded as wholly false and pernicious in the light of the results I had arrived at. It happened that when I was writing that book Dr. R. S. Woodward, President of the Carnegie Institution of Washington, was a frequent visitor at our house, and we were in the habit of discussing various questions in which we were both interested. When I reached that point in my manuscript I remember reading those pages to him and saying to him that Mr. Carnegie's idea that the Institution was to pick out exceptional men and give them their opportunity was going to give him no end of trouble, because so many would believe themselves to be the exceptional men and file their claims. But I saw that what I said did not make a very strong impression on Dr. Woodward, as he had then only recently entered upon his duties, and was thoroughly loyal to the founder. In the course of a year, however, he began often to tell me of the great number of "cranks" who were besieging him, and of men who imagined that they had discovered the key to the universe, and could unlock it if they could get a "grant" from the Carnegie Institution. I said nothing, but felt that my prediction was being fulfilled. When the book appeared I saw that Dr. Woodward had a copy, but I did not expect so busy a man could find time to read it. When this article came out I sent him a copy, and in due time I received the following letter:

Carnegie Institution of Washington,
Washington, D. C., January 28, 1908.

Professor Lester F. Ward,
Brown University, Providence, R. I.

Dear Professor Ward:

Please accept my thanks for a copy of the Sunday Magazine of the *New York Tribune*, containing your article, "What Brings Out Genius?" I had read this article with interest when it appeared in the Sunday Supplement of the *Washington Star;* but I am glad to get an additional copy which I am now filing for reference in the office of the Institution.

You have made a distinct contribution to the important question of the origin of mental capacity, often unfortunately called genius.

I hope this article will be widely read, for one of the greatest obstacles to progress at present arises from the fact that people in general think talent is something that comes from necromancy or some other form of occultism. Hence the widely general application of the term "wizard" to men of exceptional insight and productivity.

The prevalence of these false ideas gives me no end of trouble in dealing with men who consider themselves, and whom the world takes at their own estimate, as "exceptional men." Thus I am continually dealing with the stupid fallacy that a man who has never given any special attention to physics, chemistry or biology is qualified by reason of a sudden accession of enthusiasm to surpass all men learned in these subjects. A large fraction of my time is taken up in dealing with these people; but of the thousands with whom I have been in correspondence or to whom I have given interviews during the past three years, not one has contributed anything of importance to the advancement of knowledge.

The prevalence of these false ideas helps also to stimulate the paradoxers, as De Morgan called them, the men who are trying to trisect an arc, to square the circle, or to devise some sort of perpetual motion machine. Indeed all of the old humbugs are being revived again and again. Probably there never was a time when men with money to invest could be more easily hoodwinked by quacks and charlatans, who promise quick monetary returns, than the age in which we live. Hence the great educational value of an article like the one you have published, in calling attention to the real source of knowledge and progress.

With best wishes,
I am
Very truly yours,
R. S. WOODWARD.

On January 30, I wrote him the following letter:

BROWN UNIVERSITY,
PROVIDENCE, R. I., January 30, 1908.

MY DEAR DR. WOODWARD:

I am writing while the spirit moves, because if I postponed it even a day I probably would not write at all, and perhaps we may both regret that I did not do so.

If I answered all the letters I get about that "Genius" article I should not do much else for a while, but yours is of course highly exceptional, not only from its eminent source, but also from its contents.

I have abundant reason to believe that the article has been widely read. It appeared simultaneously in the Christmas number of nine of the leading papers of the country in as many large cities. It was an ordered syndicate article, but is not the kind of article they wanted. They wanted an article full of glowing laudation of American self-made men, and specified that it must be "picturesque and anecdotal!" I told them that I could not write any such article. They would not take no for an answer and the negotiations hung on all summer. Finally, the editor, a Harvard graduate and bright fellow, called on me here, and I convinced him that the Sunday Magazine was making a mistake in publishing nothing but trash, that there was a large class that wanted something else.

I wrote the article in one evening and it was in type before the end of August, but they held it for their Christmas number, which may have been a compliment.

The article is nothing but a boiling down of chapters IX and X of *Applied Sociology*, where the proofs of my position are supplied as far as space would permit. If any one wants more let him read Odin's book: *Genèse des Grands Hommes*.

While writing my book I remember discussing some of the points with you, but I felt that you were not impressed. One needs to get steeped in such a subject before it takes possession.

I am particularly interested in what you say about "exceptional men." I was very much afraid that Mr. Carnegie's use of that phrase might have just the effect you describe, and I paid my respects to the doctrine on page 266. How is it possible to tell an exceptional man before he has done anything?

I agree with you that "genius" is not the right word, and that *talent* comes nearer to it (nor "ability," as Galton says). One of my critics, Mr. Wm. H. Babcock, Patent Attorney, 604 F St., who is something of a genius himself, author of several books of poetry and fiction, and whom I have long known, has pointed this out very clearly, and refers to the etymology of the words, viz., that *genius* is derived from *spirit*, and is an airy intangible thing, while *talent* was a Greek weight and subsequently a heavy coin, and is something material, solid, and real.

The popular idea of genius is a pathologic quality, and while it produces prodigies, it also produces cranks. Your phrase "mental capacity" hits it squarely. The quality we are after is that which renders its holder capable of *doing something.*

But while I was told to write an article on *genius*, and yielded that point by calling this quality by that name, still, I think I made clear what I meant by it. I named the article: *Genius and its Creation*, and am not responsible for any part of the heading of the article as printed. The first two paragraphs are also somewhat garbled and not exactly as I wrote them. The rest is literally in my own words. The changes were made after the last proof left my hands.

I cannot get rid of the idea that this question of how mental efficiency may be consciously increased is the most important one that can occupy the human mind. It means the purposeful acceleration of human progress. But as I pointed out in the book, we need a complete statistical basis for it. Odin has furnished a model, necessarily limited to one country, one period, and one kind of mental production, and what is wanted is to extend this to all countries, all times, and all kinds of work. But this would be a vast undertaking. No single individual can hope to accomplish it. It ought to be the work of the state, of society itself, which is to be the beneficiary. But, as I said (pp. 221–223) since the state is not sufficiently enlightened to see its importance and vote its financial support, any great heavily endowed private institution might successfully conduct such an enterprise in the interest of civilization at large. It would require a great permanent bureau with an enlightened staff and a broad-minded man at its head, to lay out the work on the broadest lines and prevent its running into narrow grooves. But alas! the schemes of a thousand influential special pleaders would have to yield to it, and this cannot be.

I shall be in Washington during the spring recess in March for a week or ten days, and shall hope to see you and perhaps we may get a chance to "swap lies," as you call it for an hour.

Remember me kindly to Mrs. Woodward.

Cordially yours,
Lester F. Ward.

He replied on February 4, in the following terms:

Carnegie Institution of Washington,
Washington, D. C., February 4, 1908.

Professor Lester F. Ward,
Brown University, Providence, R. I.

My dear Professor Ward:

Many thanks for your illuminating letter of the 30th ultimo, which has not only in itself furnished much food for thought but it has led me to look up the references in your book, all of which I ought to have read before, instead of spending my time in wading thru the morasses of inane literature furnished me by "exceptional men."

Almost all of the experience the Institution has thus far acquired helps to substantiate the position you have taken with regard to the genesis of men of marked mental capacity, and the very general lack of an appreciation of the fundamental ideas which lie at the basis of this subject constitutes a serious obstacle to progress in the Institution. The prevailing disposition to look upon exceptional mental capacity as a "pathologic quality" simply blocks the way to anything like rational development of methods of procedure.

In this matter as in many others, however, there is some comfort to be drawn from the fact that human nature is rarely logical to the point of absurdity; and while we may not be able to adopt a rational course of procedure, we may yet be able to steer clear of the absurdities to which the prevailing erroneous theory would lead. Thus I am finding with regard to a number of matters that while my carefully wrought theories are not approved, my practice generally wins a sufficient number of votes to prevent its displacement.

I shall look forward with pleasure to having a chat with you when you visit Washington in March, altho I shall probably be away from the city during part of that month.

Sincerely yours,
R. S. WOODWARD,
President.

I desired very much to make the above correspondence a part of the history of this article, but I had no copy of my own letter, and I was not sure that Dr. Woodward would like to have his published. I spent the holiday week of 1910 in Washington, and while there was invited by Dr. and Mrs. Woodward to dine with them on December 30. I had taken his letters to Washington with me on purpose to bring the matter to his attention, and while at dinner I recalled the whole subject to his mind and showed him his letters. He said he had nothing to retract or alter in the least, and was more than willing I should make that use of his letters. Furthermore, he offered to have a copy made of my letter and sent to me. I found it in my mail when I returned to Providence, with the following letter from him:

CARNEGIE INSTITUTION OF WASHINGTON,
WASHINGTON, D. C., December 31, 1910.

PROFESSOR LESTER F. WARD,
BROWN UNIVERSITY, Providence, R. I.,

DEAR PROFESSOR WARD:

Herewith I am sending you a copy of your letter addressed to me under date of January 30, 1908, and I take this opportunity to reaffirm over my signature my permission accorded you to make such use as you may desire of the two letters from me in reference to the general subject of talented men and their discovery.

With best wishes for the New Year,

I am
Sincerely yours,
ROBERT S. WOODWARD.

Sunday Magazine, December 22, 1907, pp. 11 and 14.

WHERE do great men come from, and how do they arise? Is it a matter of inheritance, opportunity, of training, or just sheer luck? Why do more great men occur in one age than in another? Can their numbers be increased by cultivation? Do the well fed grow into geniuses more often than the starved? These are fair questions. Nobody claims they are new: for they have been asked by every generation since Socrates. The answers now given are based on long study by the foremost authority on sociology in America.—EDITORIAL NOTE.

IT goes without saying that civilization—whatever it is—has been made by men. But it wasn't always taken for granted. A great many people held the view for awhile that civilization was the work of certain blind, vague agencies of nature. They got into that way of thinking by opposing the view championed by the great historian Carlyle in his "Heroes and Hero Worship." He maintained that demigods and heroes had made all our history. This seemed to many persons unscientific, and they took the other side.

Now the common sense view is coming back. As John Morley puts it, if men were all to fall into a Rip Van Winkle slumber for the space of a generation, they would awake surprised. They would find that the world had stood still during all that time, and would not resume its forward movement till they resumed their efforts. The same truth is proved by history, as when a nation—Spain, for example—sees fit to destroy or drive out its progressive spirits, its march is arrested and it falls into intellectual decay. And conversely, a country, such as Switzerland, which becomes an asylum for the victims of spiritual persecution, soon towers above all other lands in the display of genius. The conclusion is irresistible that the agents of civilization are men.

Not only is this true, but it is also clear that human progress is the work of genius; and therefore it is no wonder that great minds should regard as the problem of problems that of increasing the product of genius. But it is here that we arrive at the parting of the ways. One school contends that all the genius in the world is always in full action, and that the only way to increase its salutary effects is to increase its quantity in society. This is the doctrine of the irrepressibility of genius, and, as all now admit that genius is hereditary, it is seriously proposed to adopt a wholesale system of conscious and purposeful selection of superior progenitors of the race. But as breeders always breed for points; and as in man genius is regarded as the great "point"; furthermore, as genius is a function of the brain, and therefore brain increase is the great desideratum,—if this plan could be carried out we might have at length the race of men described by Mr. Wells: all head and no body. It therefore is probably fortunate that no such scheme can be forced upon a race of intelligent beings.

RISE OF THE OPPONENTS

But this doctrine that genius is a fixed quantity, all of which constantly asserts itself, has now been successfully challenged, and an opposite school has arisen which declares that the greater part of all the genius of the race lies latent, and always has lain latent, and that all manifestations of genius are due to favorable circumstances of one kind or another that have made it possible for a relatively few of the indefinitely great number who actually possess genius to burst through the thick crust of conventional deposits that overlie the world. The statistical method, so freely employed by the hereditarian school, has been turned upon it in support of this later view, and with the most astonishing results. It is now proved that heredity is only one, though an essential one, of a great number of influences conducive to the manifestation of genius, and that without such other influences no degree of native genius can avail. These non-hereditary influences are of many kinds; but they may all be classed under one great group, and distinguished as environmental. In other words, the display of genius depends upon the two coöperating factors, heredity and environment, and a favorable environment is quite as essential to success as the native powers themselves.

A further analysis shows that the kind of environment which is effective either in the expression or the repression of genius is an artificial environment, that which society supplies and imposes, and not the natural environment under which the born genius lives. The physical conditions by which he is surrounded—climate, soil, mountains, lowlands, sea coasts, plains—count for little in determining his success. *Cælum non animum mutant qui trans mare currunt.* [They that roam change their skies above them, but not their hearts.] Even the race to which he belongs, the current belief to the contrary notwithstanding, has been found to be an almost negligible factor wherever a thorough test has been applied.

THE ALL-POWERFUL FACTORS

THE influences that count are always human or social influences, and these admit of a rough classification. Thus there are four kinds of artificial environment that affect directly the genius of man,—stifling it if unfavorable, stimulating it if favorable, and thus effectively determining its failure or its success. These are (1) the local environment, or the contact of men and things; (2) the economic environment, or the material means of subsistence; (3) the social environment, or the social class to which the possessor of native genius belongs; and (4) the educational environment, or the kind and amount of education that the born genius has received, especially in youth. These are the great, all-powerful factors in determining the expressions of latent genius, and unless one or more of them are favorable there is no possibility of such expression, however great that latent genius may be.

Statistical demonstration of this truth has been supplied for literary genius throughout modern times, especially for France, but also less fully for Italy, Spain, Germany, and England. A few of the general results are so striking that they cannot fail to command attention.

For the local environment, the great factor is found to be the cities as compared with the rural districts. Thus the number of eminent Frenchmen of letters born in cities was seventy-seven to the hundred thousand inhabitants, while those born in the country were only six to the hundred thousand, showing that the literary productivity of cities was nearly thirteen times that of the country. That of Paris was between eight and nine times that of the whole of France, and more than thirty-five times that of the rural districts. *Lex urbis lex orbis.* [The law of the city is the law of the world.] It is the material conditions which cities supply that stimulate and bring out the potential genius of mankind.

The influence of the economic environment is even more powerful. It has been shown that only nine per cent. of the eminent literary men of France were of limited means, while ninety-one per cent. were rich or well to do. But as the poor formed ninety-seven per cent. of the population, it follows that three per cent. of the population furnished ninety-one per cent. of the talent. This means that the chances of success of a rich person are more than three hundred times as great as those of a poor person endowed with the same measure of native genius.

The social environment is closely connected with the economic, and the results are practically the same. In France during the modern period the upper classes—nobility, Government officials, liberal professions, bourgeoisie—furnished ninety and two-tenths per cent. of the eminent men of letters, while only nine and eight-tenths per cent. were low born. But these latter constituted at least eighty per cent. of the population. It follows that the chances of success of a person belonging to the upper classes are nearly thirty-seven times as great as those of one of the same native ability belonging to the lower. But the nobility alone, who constituted only about one per cent. of the population, furnished twenty-five and five-tenths per cent. of modern Frenchmen of letters. The chances therefore are nearly two hundred times as great for a person belonging to the nobility as for one belonging to the working class, all other things being equal.

VALUE OF EDUCATION

Finally, as regards the educational environment, notwithstanding the claim openly made that for men of genius education is a negligible factor, it was found that out of eight hundred and twenty-seven Frenchmen of letters, the only ones whose early history in this respect could be ascertained, eight hundred and eleven received a thorough education in their youth, while only sixteen were self educated. It follows that for an educated man, other things equal, the chances of success are nearly fifty-two to one for a man who is compelled to educate himself.

Similar statistical tests have been made for the four other great literary countries of Europe, and the results do not vary essentially from those attained for France. The influence of great cities, of ample means, of high social position, and of early education is the same everywhere. But it can be readily seen that all these environments are educational. They are the means to education. It was found that the size of cities had less influence than other qualities, and that the most potent qualities were their educational facilities. It is true that Paris towers above all others; but aside from its population it is also the great seat of learning of Western Europe. Many large commercial cities, such as Havre, Toulon, Nantes, St. Étienne, Nice, rank very low in literary productivity, while other relatively small ones, especially the fifteen university cities of France, have been exceptionally fertile in literary talent. Of these, Dijon, Besançon, Grenoble and Aix are striking examples. All environments are favorable to the development of genius exactly in proportion as they are educational in the full sense of the word.

The thoughtful student of such facts as these cannot fail to perceive that genius is something that exists and only requires to be developed; that the amount of native talent in society is practically unlimited, and that its manifestation in the furtherance of civilization is mainly a question of opportunity. All that is needed to bring it forth in any required amount is that there be such a demand for it as shall really develop it.

Not merely literary talent, but talent of every kind,—artistic, scientific, practical,—lies latent in every land, waiting to be called forth.

Great astonishment has been expressed at the exuberant talent of the Greeks when that country was at the zenith of its history. The galaxy of historians (Herodotus, Thucydides, Xenophon), statesmen (Lycurgus, Solon, Pericles, Cimon, Themistocles), orators (Demosthenes, Isocrates, Lysias), warriors (Miltiades, Nicias), sculptors (Phidias, Praxiteles, Scopas, Myron), painters (Zeuxis, Polygonotus), architects (Mnesicles, Ictinus, Callicrates), philosophers (Socrates, Plato, Aristotle), poets (Sophocles, Euripides, Aristophanes), and dramatists (Æschylus), that illumined the Grecian skies from the sixth to the fourth centuries before the Christian era has been the marvel of modern writers, and has led many to maintain that far more genius then existed than has ever existed since.

MUST BE A REWARD

But the true explanation is not difficult to find. It was simply due to the fact that Greece in that day, more than any other country at any later age, actually called out its talent and set it to work. Genius, like economic goods, is subject to the law of demand and supply. If it is demanded, it is forthcoming, and in any required amount. But the demand, as in political economy, must be a real, substantial demand, and not a mere velleity or a vain craving. No demand will be supplied unless it includes the wherewithal to labor and subsist. In the economic world the mere desire for or even the need of a commodity does not constitute a demand. An economic demand involves not only the wish for the thing demanded, but also the ability to obtain it in the market. And so a demand for the products of genius must be accompanied by the assurance that the man of genius shall be adequately remunerated for his services. He must be enabled to throw himself into his work without being haunted by the specter of want. It was this that Greece did, with the result that this substantial demand was abundantly supplied. Rome did something of the kind in the Augustan age. The Renaissance was largely a product of a serious demand on the part of both Church and State for great works of art. Even in medieval times and for several centuries later there was no lack of architectural talent to build the many great cathedrals and castles of Europe.

It is the same to-day. Every kind of genius for which there exists this real, living demand is even now at work. The supposed decline of literary and artistic genius is due entirely to the fact that those forms of genius are no longer demanded in this sense. But other forms of genius are in demand, and the demand is supplied. Genius is Protean in its nature. The inventiveness, which in the eighteenth century was expended upon fine figures of rhetoric, is to-day expended upon practical problems of life.

When the Department of Agriculture calls for experts in the prevention of the diseases of domestic animals and cultivated plants or in the discovery of improved fertilizers, they come forward in great numbers, and when set to work they display the highest genius, and soon devise means of saving the nation millions of dollars. They go beyond all that was expected of them, and invent processes for increasing wealth. In the study of soils they find ways, such as the introduction of nitrobacteria, of doubling or quadrupling the fertility of the land. Like results are obtained in other great practical undertakings: the development of mineral resources, the study of geological formations, the scientific mapping of the public domain, the great geodetic operations so useful to commerce and industry, the chemical investigation of the mineral, vegetable, and animal products, nutritive and medicinal, foreign and domestic. And along with this practical work the redundant genius of these trained experts always finds means of making strictly scientific discoveries and ingenious inventions never contemplated by the State, and thus of indefinitely expanding the domain of science, art, and industry.

SUPPLY WOULD FOLLOW DEMAND

Such are some of the ways in which the genius of to-day is displaying itself. These are the fields in which the demand exists, and it is always supplied. But I venture to say that if a demand of this substantial character existed for literary productions, for poetry and drama, for art in any form, sculpture, painting, ceramics, musical composition, it would be supplied in like manner, and would in every department prove to be of a high order. The age of Pericles or Augustus could be reproduced, or perhaps surpassed, and almost immediately, by the simple process of calling out the latent genius of this or any other modern country in the same manner in which it was then called out. Genius is universal, all sided, unlimited; but the greater part of it is always latent for the reason that it is not demanded.

If the display of genius is to be increased, it must be done by creating a market for the products of genius. Intellectual production is subject to the same laws as economic production, and the spiritual resources of the world are as inexhaustible as are its material resources.

But the State should not be the only appraiser of genius. Society is its proper appraiser, and until society shall appreciate its geniuses and learn to reward them, few of them, relatively speaking, will ever come forward. The true market for achievement is the people, and when the people themselves shall demand it the supply will promptly meet the demand. The true way, then, to call out the dormant genius of mankind is to create this great popular demand, this world market, into which all the talent of the race will pour. How can this be accomplished? Only by the spread of ideas and ideals. There is no royal road to this goal, and the discussion of the ways and means transcends the limits of this article.

It has been maintained that a great difference exists in this respect between men of action and men of thought, between warriors and statesmen on the one hand and philosophers and scientists on the other. They tell us, and truly, that the Alexanders, Cæsars, Napoleons, Nelsons, Washingtons, and Grants, as well as the Solons, Catos, Richelieus, Gladstones, Websters, and Lincolns, have been simply the products of their respective times and countries, and would never have been heard from if they had lived under different political conditions. But we are assured that such men as Aristotle, Pliny, Galileo, Descartes, Leibnitz, Newton, Darwin, Franklin, Agassiz, and Marsh were the possessors of an intrinsic genius which is independent of circumstances. The same claim is made for literary and artistic geniuses as well as for renowned inventors.

This has been shown not to be the case. The law applies to all forms of genius. Most of those mentioned and scores of others have been investigated, and it has been found that without exception they have all owed their fame to the combination of genius and opportunity, that besides being native geniuses they have in a proper sense been called upon to display their talents in the direction in which they have displayed them.

AMERICAN GENIUSES THE SAME

When few systematic researches of this kind have been made for the leading American geniuses of any class, there is no reason to suppose that they are exceptions to this universal law, and upon those who maintain that they are such rests the burden of proof. That this law holds good for our generals and statesmen is too obvious to need stating. But for the Civil War, Grant, though he had a military training, would have remained an obscure tanner in Galena; Sherman, Sheridan, Custer, and the rest would have spent their lives at various outposts in the far West, known only to military circles. But for the great political questions that shook the country before that war, brought it on, and carried it through, as history records, even Abraham Lincoln would have never done more than grace the Springfield bar.

It would be easy, if space permitted, to take up other classes of eminent Americans and show how in one way or another the particular environment in each case was the decisive factor in their success. For example, Benjamin Franklin, though in somewhat straitened economic circumstances in his youth, was a printer by trade, and thus always able to earn a livelihood. But the printer's is the one trade which has for its natural effect to bring its possessor into direct contact with the great men and great thoughts of the age. The average journeyman sets up the type from the copy before him without imbibing the thought that it expresses, but for a genius like Franklin every type touched with his finger is fraught with meaning, and a printing office is a university. If Benjamin Franklin had learned the shoemaker's instead of the printer's trade, he never would have captured the thunderbolt.

Anything connected with the press proves highly educative, even the selling of newspapers; and, as Elbert Hubbard has said, one never knows what future may lurk in a half-clad newsboy crying his wares. It is probable that the world is indebted to this "profession" for an Edison. But here, and not less in the case of Alexander Graham Bell, true genius found itself in the presence of a great public demand for its products, and such a combination knows no failure.

December 29, 1907—Ætat. 66.

538. Social Classes in the Light of Modern Sociological Theory. Abstract

History.—Written December 20, 1907. Knowing that an abstract of my address (see infra, No. 540) would be wanted for the press, I drew it up somewhat carefully before leaving for Madison. On December 26, when I called on Dr. Thwaites, I handed it to him to use as he might see fit. I saw no newspapers while in Madison, and after my return to Providence I wrote to Dr. Thwaites to ascertain whether it had been published. He replied that it had, but gave me no definite references, so I wrote to Dr. Ross, who kindly hunted it up and sent me copies.

The Madison Democrat, Vol. LXXVIII, No. 308, December 29, 1907, p. 1.

January 25, 1908—Ætat. 66.

539. The Sociology of Political Parties*

History.—Written February 22, to June 6, 1906. Much of the history of this English draft was unavoidably given with that of the French paper (supra, No. 534), and need not be repeated here. I took the rough draft with me to Paris and let Mlle. Borgès use it in correcting the French. After the other was completed, viz., on May 31, thinking it possible that I might have to read my paper in English at the London Congress, I commenced making a fair copy of it in a large round hand suitable for that purpose. This was finished on June 6, and I kept it with the French article, but did not have to read it (see supra, No. 533). The *Annales*, Tome XI, containing the French article, reached me on June 6, 1907, and as it was then permitted by the statutes of the Institut to publish a translation, I wrote to Dr. Small with the result stated in the history of the other paper (see supra, No. 535), and both papers were sent at the same time. Proof of this one was received on July 5, but it did not appear till January, 1908, the number containing it arriving on the 27th of that month. I did not get my reprints till February 15.

The American Journal of Sociology, Chicago, Vol. XIII, No. 4, January, 1908, pp. 439–454.

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* From the *Annales de l'Institut International de Sociologie*, Tome XI, pp. 393–417. Paris, 1907.

March 31, 1908—Ætat. 66.

540. Social Classes in the Light of Modern Sociological Theory

History.—Written October 2, to November 26, 1907. On December 28, 1906, I learned that the committee that I had appointed to nominate officers of the American Sociological Society for the year 1907, had decided to nominate all the old officers for a second term. I therefore absented myself from the meeting at which they were to be elected, but this did not prevent my being reelected president of the Society. The explanation given, and doubtless true, was that there was danger of too soon running out of "presidential timber." As the year advanced it became incumbent upon me to prepare another address. The subject of castes and social classes had been interesting me, and I had decided to devote my address to that subject. When therefore the Secretary, working up the program for the annual meeting, asked me on September 19, what would be the title of my address I gave it simply as *Social Classes*. On October 2, I commenced blocking it out. By the 9th I had finished the rough draft. On October 29, Professor J. W. Jenks, President of the American Economic Association, with which our Society was to meet at Madison during holiday week, wrote to know what my title would be. It had been arranged that his address and mine should be the same evening at a joint meeting. On November 1, I wrote to him and also to Dr. Veditz giving them the full title as I had then formulated it. On the 10th of that month I revised the rough draft prefixing six pages to the original and changing the title to its final form. On November 23,

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* Address of the president of the American Sociological Society at its second annual meeting in Madison, Wis., December 28, 1907.

I gave the whole a second revision and commenced writing the final draft as I intended to read it. I finished it on the 25th and revised it on the 26th. In that form it was delivered on December 28th. Ex-Senator Vilas presided at the meeting and made a short speech; Professor Jenks's address occupied forty minutes, his subject being practical and popular. Mine was an abrupt change, but the audience seemed to make it easily and listened attentively. On the 30th I gave my address to the Secretary who arranged with Dr. Small for its publication. Proof was received on February 14, 1908, and the number of the *Journal* containing it on April 2. My reprints arrived on April 20. Vol. II of the *Publications* of the Society appeared about the end of June, my copy reaching me on July 2.

This address, like its predecessor (see supra, No. 530), was published in full in the *Plebs Magazine.*

The American Journal of Sociology, Chicago, Vol. XIII, No. 5, March, 1908, pp. 617–627; Publications of the American Sociological Society, Second Annual Meeting held at Madison, Dec. 28–31, 1907, Vol. II, Chicago, 1908, pp. 1–11; The Plebs Magazine, Oxford, Vol. II, No. 7, August, 1910, pp. 165–168; No. 8, September, 1910, pp. 191–196.

March 31, 1908—Ætat. 66.

541. Motives in Social Conflict

History.—Remarks on the paper of Professor T. N. Carver on *The Basis of Social Conflict*, read December 28, 1907. Stenographic report corrected February 5, 1908.

The American Journal of Sociology, ibid., p. 646; Publications of the American Sociological Society, ibid., p. 30.

May 31, 1908—Ætat. 66.

542. [Social Integration through Conflict]

History.—Remarks on the paper of Professor Franklin H. Giddings on the question: *Are Contradictions of Ideas and Beliefs likely to play an important Group-making Rôle in the Future?* read December 30, 1907. Stenographer's notes corrected January 27, 1908. The first two paragraphs have nothing to do with the subject I rose to discuss, but were called out by the remarks of the preceding speaker, Mr. F. A. Gilmore, Pastor of the Unitarian Church in Madison. His remarks as they are reported in the Proceedings of the Society may seem germane to Professor Giddings's paper, but as he made them they were wholly irrelevant, and consisted in an attack on my views as expressed in my books relative to the rôle of religion in society. I have, however, spoken more definitely on this subject in two of my articles than in any of my books (see supra, Nos. 444 and 535). The latter of these I sent to Mr. Gilmore.

The American Journal of Sociology, ibid., No. 6, May, 1908, pp. 806–807; Publications of the American Sociological Society, ibid., pp. 188–189.

July 10, 1908—Ætat. 67.

543. Review of Social Psychology: An Outline and Source Book: By Edward Alsworth Ross. New York, The Macmillan Company, 1908. Pp. xviii + 372.

History.—Written June 17–22, 1908. I was aware that Dr. Ross was writing this book, and on December 27, 1907, when I was at Madison, he read me two chapters of the manuscript of it. On June 6, 1908, I received a copy from the publishers, and I commenced reading it at once. I thought I would review it, and on the 8th I wrote to Dr. Cattell to inquire whether he would care for the review in *Science*. He replied that he would be glad to have it and as soon as I had finished reading the book I wrote the review. On July 5, I received a copy of the book from Dr. Ross, duly inscribed to me, but I had already read the proof of my review, and it appeared on the 10th. I sent him a copy of *Science* containing my review and received a letter from him too flattering to quote, but I was glad to hear him say:

> "I enjoy greatly what you say about the psychologists and their 'jargon.' If they had been sensible and enterprising they would have annexed social psychology long ago. And if they had shown any capacity to cover the field I should never have thought of homesteading it."

Science, New Series, New York, Vol. XXVIII, No. 706, July 10, 1908, pp. 54–56.

IT must have required considerable courage on the part of Dr. Ross to venture a new book on social psychology. For although he says in the preface that "the ground is new," still, as he well knows, and as his materials show, the subject itself is very old and has been worn threadbare.

The only thing that could be done, and the thing that he has virtually done, was to undertake a new compilation of the matter already extant. For, without making a count, it seems safe to say that fully one half of the matter of the book is between quotation marks, and its character as a compilation would have been apparent if all the citations had been printed in different type. But this is very far from being a criticism of the book. Indeed, under the circumstances it is its highest commendation.

And yet he has by no means utilized all the literature. Professor Sumner's "Folkways" reached him too late for use, but it would have been an inexhaustible source of facts for such a work. One of the most important omitted works is Michailovsky's elaborate treatise on "The Heroes and the Crowd," which first appeared in *Russian Wealth* in 1882 and was republished in a collection of essays in 1896.[1] In this essay imitation and suggestion are ably handled, and many of Tarde's best thoughts are anticipated. The religious epidemics of the middle ages are described in detail, and contagious manias of suicide and homicide are fully treated. The subject of the influence of the mind on the body, now brought into such prominence by christian science, received special attention, not merely in recording the alleged instances of "stigmata," but in enumerating many other illustrations. In no other work, so far as I know, is the case of Jacob's "ring-streaked, speckled and spotted" sheep and goats referred to this principle, not only as illustrating its effect on animals, but as showing that it was understood by Jacob and effectively acted upon.

Another of the older, much neglected works is Carpenter's "Mental Physiology," 1875, which deals in a scientific way with many of the psychic phenomena now referred to social psychology. Carpenter laid great stress on the principle which he called "expectancy," which is really none other than that now perhaps less happily called "suggestion."

But of course Tarde's works stand out as the leading contributions to social psychology, and it is refreshing to see them prominently recognized by Dr. Ross in the preface to this book. It has become so much the custom of American writers, while reiterating the truths they contain, to ignore their source, that this manly acknowledgment will be appreciated by all admirers of the great French sociologist.

Dr. Ross well says that social psychology is not the same as psychosociology. It is not the psychology upon which sociology rests, and which furnishes society with both its motor power and its guidance. That is an entirely different and far more important science. Social psychology is the science of the mutual influence of psychic phenomena. Mr. H. G. Wells has properly described it as "an exhaustive study of the reaction of people upon each other and of all possible relationships."[1]

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[1] "Heroi i Tolpa," Russkoe Bogatstvo, 1882; Sochineniya, Vol. II., St. Petersburg, 1896, pp. 95–190.

Social psychology, thus understood, has been treated by all kinds of writers. Very little of value has been contributed by the psychologists proper. When they approach it they load it with such a mass of technical terms, borrowed from their psychological "jargon"—dialectic, ego, alter, socius, eject, project, subject, etc., with the innumerable derivatives of these terms, that, however commonplace such ideas may be, the reader's mental stomach is so turned by their pedantic iteration that it is incapable of following what little thought they may represent.

But many writers besides Tarde have treated special aspects of the subject with clearness and force. Among these Dr. Ross himself must be counted and placed in the front rank, for his "Social Control" and other writings deal primarily with social psychology. In the present work he lays under contribution a great array of authors and a vast literature. No attempt can be made here to summarize this body of knowledge. The arrangement of the material is the original part of the work, and this could not probably be improved upon.

In some of the later chapters Dr. Ross has been able to free himself more fully from his historical bearings, and to strike out into fields more distinctly his own. This is especially the case with Chapter XV, on the Relation of Custom Imitation to Conventionality Imitation, and Chapter XVI, on Rational Imitation, which is the coming form of imitation based on intelligence and scientific knowledge. Chapter XVIII, on the rôle of Discussion, is also luminous, and pushes the subject some distance beyond the point where Bagehot left it. Chapter XXI, on Compromise, is all too brief, and John Morley is not mentioned.

The final chapter (XXIII), on Disequilibration, deals with invention (in the Tardean sense), and displays an astonishing grasp of the progress of human thought. No one has better shown how it is that premature discoveries lie dormant till the world is ready for them. Under the heading that "the higher the degree of possibility, the sooner the invention is likely to be made," he says (pp. 359-360):

> The inventions (or discoveries) in a particular field—and often those in different fields—are in a chain of dependence which obliges them to occur in a series. Each ushers in a train of possibles. Now, when no intervening invention needs to be made, an invention may be said to be in the *first* degree of possibility. When it is contingent on another yet to be made, it is in the *second* degree of possibility. And so on. Now, when an invention or discovery reaches the first degree of possibility, it is *ripe*. Thus, after Kepler announces the laws of planetary movement, the discovery of the principle of universal

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[1] "A Modern Utopia," New York, 1907, p. 83.

> gravitation is in order at any moment. After Galileo has proclaimed the laws of the pendulum, its use in time-keeping needs but a single stride. The electric telegraph is due any time after Ampère's discoveries. The invention of Crookes's tubes brings the X-ray into the foreground of possibility. After the discovery of the Hertzian waves, a few short steps bring wireless telegraphy upon the scene.

And in showing "how society can promote invention," he significantly adds (p. 360):

> The difficulty of making the combination of ideas for any particular invention will depend upon the number of persons who possess these ideas, and on the frequency in this number of individuals with the intellectual capacity necessary to combine the ideas into the invention. There is no way of affecting the latter condition, for the genius is in no wise a social product; but organized society can affect the former condition. A universal system of gratuitous instruction with special aid and opportunities for those who show unusual power amounts to an *actualizing* of all the potential genius in a population, and is the only rational policy for insuring a continuous and copious flow of inventions. It is hardly necessary to point out that only a stimulating, equipping education can mature geniuses. A régime that prunes, clips and trains minds levels genius with mediocrity. A schooling devised primarily to produce good character, or patriotism, or dynastic loyalty, or class sentiment, or religious orthodoxy may lessen friction in society, but it can not bring genius to bloom. For this the prime essentials are *the communicating of known truths* and *the imparting of method.*

On the whole we have in this work an able marshaling of the knowledge thus far brought to light on the subject of social psychology, and a clear, untechnical, while at the same time often eloquent, discussion of the laws, principles and leading truths of that rather subtle and recondite branch of sociology.

544. The Value of Error

History.—Written June 30, to July 1, 1908. In 1905 I was made one of the Honorary Associates of the Rationalist Press Association of London. I was assured that there were no duties attached, but on May 16, 1908, I received a letter from the Vice-Chairman, Mr. Charles A. Watts, asking me to contribute an article to the next *Annual*. It seems that the other Honorary Associates had contributed, and it was desired that all do so. But up to that date I had never seen a copy of the *Annual* and had no idea what it was. I replied to that effect on June 5, and on the 25th I received a copy of the last issue. I then wrote this article and sent it on. It was gratefully acknowledged and a proof sent me on August 24, also reprints that I had asked for arrived November 2, but no copy of the *Annual* for 1909 was sent. My article seemed to be the first in the number, but I could not tell from the reprints.

I was in London on July 30, 1909, and I went to the rooms of the R. P. A. in Johnson's Court. No one was there but a young lady, but she gave me a copy of the *Annual* containing my article It is the first, but the reprints are separately paged.

The Rationalist Press Association Annual and Ethical Review, 1909. Edited by Charles A. Watts. [Second Year of publication] London: Watts & Co., 17, Johnson's Court, Fleet Street, E. C., pp. 5–9; reprint, pp. 3–7.

December 25, 1908—Ætat. 67.

545. The Career of Herbert Spencer

History.—Written November 26, to December 1, 1908. I have already mentioned (supra, No. 500) Dr. Cattell's request in 1904 that I write a memorial article on Herbert Spencer for the *Popular Science Monthly.* I had recently written one on his Sociology (see supra, No. 498) and I then reviewed his *Autobiography*, and I did not have time to do more. But after the *Life and Letters* appeared in 1908, I felt that the hour had come for me to speak if I spoke at all. Accordingly on October 23, I wrote to Dr. Cattell offering to review that work in *Science*, intending, if I did so to make it even more extended than I had made the review of the *Autobiograhy.* On the 25th he wrote me that he had already asked Professor Wenley of the University of Michigan to review the work. He expressed regret at not thinking to make the proposition to me, and thought perhaps Professor Wenley would still be willing that I should do it, and he added:

> "I should, however, be particularly glad to have an article on the subject in the *Popular Science Monthly.* Spencer contributed about a hundred articles to the *Monthly*, and it was indeed for many years practically the organ of the Spencerian philosophy. A tribute to him there would be especially welcome."

I answered indicating a willingness to write the article for the *Monthly*, and asking him to send me the work, which I had not yet seen. In his reply of October 30, he stated that he would have the work sent to me, and enclosed an article that he had himself written and published in the September number, reproducing three of the portraits of Spencer that adorn the *Life and Letters.* It arrived on November 14, and I read it in eight days. I spent several days preparing slip notes similar to those made for my review of the *Autobiography*, in fact using these so far as the heads were the same. I also followed the same plan here as in the review of preparing a rough pencil draft. This was finished on November 29, and the final draft begun the same day. It was finished on the 30th, revised on December 1, and sent to Dr. Cattell on December 2. On the 5th he acknowledged it, accepted it gratefully, characterized it as "admirable," and offered me as many reprints as I desired. Proof was read on the 10th, and the *Monthly* with it in on the 26th. This was the number for January, 1909, which begins Vol. LXXIV, and my article is the first in that number. It therefore stands at the head of the number, of the volume, and of the year. Nothing more surely could be asked or desired. Reprints arrived on January 9.

There were some echoes from it, especially from across the seas. Lord Avebury wrote under date of January 27, 1909, that he had read it with much interest, and added:

> "I should have put the *First Principles* perhaps rather higher than you do, but I agree with you that the *Principles of Biology* is the gem of his *Synthetic Philosophy*."

Mr. Victor V. Branford, Honorary Secretary of the Sociological Society of London, under date of February 2, wrote:

> "I think you have evaluated Spencer's position in true historical perspective. Personally, too, I am quite of the opinion that by far the most important of his books was the *Biology*, and indeed, when I ask myself what first rate contribution—contribution of a magistral kind—he made to Sociology, I find it hard to reply."

A certain biographical interest attaches to the letter of Mr. James Collier from Sydney, Australia, dated March 6, 1909, in which he says:

> "The article on the *Career of Herbert Spencer* has indirectly rendered me a service. In a will drawn up in 1878 Mr. Spencer appointed me his literary executor and biographer. With the total breakdown of my health and my consequent departure for the Antipodes, the arrangement naturally came to an end, and when my old friend Duncan returned from India to reside in London, he was naturally chosen. I was unaware of the fact when, some two years since, I began a critical biography of Herbert Spencer. The book was actually finished before Duncan had concluded the composition of his work, but its publication seemed to be precluded by the publication of Duncan's book. This I long refrained from reading, and thus was at the mercy of those who

told me that the volume completely covered the ground. Your article, however, sent me to the book, and now I have procured a copy of it. After carefully examining it I revert to the conclusion that the two volumes are on totally different lines. There ought to be room in the market for both a volume of memoirs like Duncan's and for a volume describing the evolution of the thinker and of his doctrines. I shall now revise my book, and I shall be greatly aided by your article, which I carry about with me as I did the *Independent* article" [*Herbert Spencer's Sociology*, previously mentioned by him].

The Popular Science Monthly, New York, Vol. LXXXIV, No. 1, January, 1909, pp. 5–18.

THAT "the evil men do lives after them; the good is oft interred with their bones," is one of those literary palindromes which may be read both ways. Probably there is no great man, who, from the standpoint of pragmatism, has not done both evil and good, but the question as to which predominates can never be decided to the satisfaction of all. In the case of a Nero most men are agreed, but in that of a Napoleon opinions differ. Aside from war and politics there are few cases in which the consensus of opinion would fall on the side of evil, but many cases leave it doubtful, as for example, those of Machiavelli, Hobbes, and Rousseau, while in others it changes from one age to the next, as in the cases of Voltaire and Thomas Paine. Usually it is the evil that is most conspicuous during the life of the subject, and this has often been carried to the extreme of persecution during life and canonization after death. The world is full of monuments to those who were put to death for the things that are now chiefly admired. All this admonishes the biographer of the caution required in passing judgment on those of his own day and generation.

Herbert Spencer stands, and will probably always stand, on the light side of the picture, but there are very few of those familiar with his work who would maintain that there is no dark side. His "Autobiography" naturally presents the bright side, but the "Life and Letters" emphasize rather the shades than the lights, and it may be doubtful whether it would not have been better if that work had not appeared. Still, when we remember the deficiencies of human nature, perhaps this showing up of the whole man as he was is nothing more than a reassertion by him of the universally approved maxim of Terence: *homo sum.*

Leaving the world, then, to pronounce its judgment on this question of ethics, or pragmatism, let us briefly consider the career of Herbert Spencer in its broad outlines as brought out by his complete works supplemented by the four posthumous volumes now before us. There is certainly no vehicle in America, if there is any in the Old World, more appropriate to this task than the POPULAR SCIENCE MONTHLY. As has already been said by its present editor:

> Readers of this journal are familiar with Spencer's work, for he contributed to it nearly a hundred articles. It was indeed established by Dr. E. L. Youmans in 1872 largely with a view to provide a suitable medium for printing Spencer's "Study of Sociology," and . . . may be regarded as one of the by-products of his genius.[1]

As is well known, the first article in the POPULAR SCIENCE MONTHLY (May, 1872) is by Herbert Spencer: "The Study of Sociology, I., Our Need of It," which is also the first chapter of the book he was writing on "The Study of Sociology," for the International Scientific Series. In his "Autobiography" (Vol. II., pp. 284–286) he says:

> Before he left England my American friend [Dr. E. L. Youmans] volunteered to arrange for the carrying out of a suggestion which had arisen, I do not remember how, that the successive chapters of "The Study of Sociology"—the extra book in question—should be first published serially, in England and America at the same time. Here the *Contemporary Review* . . . was the contemplated medium; and a fit medium in the United States, Professor Youmans proposed to negotiate with as soon as possible after his return. . . . And now there arose an unlooked for result from the understanding that had been made for simultaneous publication in America. Negotiations which Youmans had carried on with one or other periodical in the United States had all failed; and at the time when the first chapter had been put in type, neither he nor I saw how our plan was to be carried out. When the proof of this first chapter reached him it caused prompt and surprising action, as witness the following extract from a letter of his dated April 3, 1872:
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> "A thousand thanks for your favour of March 13th, with article on 'Study of Sociology' enclosed. . . . You did wisely in sending it, and I decided upon our course in ten minutes after getting it. I determined to have a monthly at once, and in time to open with this article . . . We have started a monthly of 128 pages. . . . I am utterly glad that things have taken the course they have. I have wanted a medium of speech that I can control, and now I shall have it."
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> The magazine thus started was *The Popular Science Monthly;* which, under the editorship of my friend, has had a prosperous career and done very good work.

Not only did the "Study of Sociology" thus all appear in the POPULAR SCIENCE MONTHLY, but Mr. Spencer continued to contribute to it chapters from his "Synthetic Philosophy" for many years, and declined to allow them to appear in other periodicals. When asked as late as 1895, by the London editor of *McClure's Magazine* to contribute to that journal, he replied:

> I have, in virtue mainly of my indebtedness to my old friend for all he did on my behalf in the United States, felt bound to make the *Popular Science Monthly* my sole medium for publication of articles in the United States, and

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[1] POPULAR SCIENCE MONTHLY, September, 1908. Vol. LXXIII., p. 285.

the obligation, which was peremptory during his life, remains strong after his death, since his brother occupies his place and he has continued his good offices on my behalf.[1]

The choice of a spokesman is less happy, but when we remember that the brothers Youmans, John Fiske, and most of the other disciples of Spencer in America have passed away, the difficulty in finding a proper person for such a task will be appreciated. Probably it should have fallen to an unqualified disciple who would simply pronounce an *éloge* in some extended form. The one to whom it has been assigned, while he yields to none in his high estimate of Spencer's talents and achievements, and has made this known on many occasions, has remained eclectic as to his peculiar doctrines, accepting such as appeal to him as sound, rejecting those which seem to be obviously unsound, and suspending judgment as to many that appear doubtful or await sufficient evidence.

In these several respects it is possible to classify Spencer's views under two heads and to explain the reasons which assign them to the one or the other class. The first class includes his cosmic philosophy in general, beginning with inorganic nature and extending through biology. It also includes much of his psychology and sociology, considered in their philosophic aspects. The second class embraces his ethics as a whole, both individual and political. To it also belong most of the applications that he makes of psychology and sociology to current events, his dealings with the state, government, war, industry, business and economic problems. While no one will go so far as to say that his views on the first of these classes are always sound, or that those on the second are always unsound or questionable, it is still true that all that is great and profound in his philosophy belongs to the first of these classes, while his errors, his narrow views, and his unworthy utterances are confined to the second class.

And now as to the explanation of this. Primarily it rests on the fact that in treating the first class of subjects there was no room for the play of the emotions, while the subjects of the second class often appeal to the feelings, and Spencer, with all his logic and philosophic poise, never had his feelings under complete subjection to his reason. But secondarily, in the case of topics appealing to the feelings he unfortunately imbibed a whole series of prejudices during his early youth from which he was never able to free himself. Indeed, they were so strong that he did not attempt to overcome them, but rather gloried in them to the end of his life. This, however, was not the worst consequence. They blinded him to everything that was taking place in the world around him, to the extent that social movements, which, could he have seen it, were the natural outcome of the cosmical principles he had laid down, were regarded by him as the signs and omens of social degeneracy and as portending a relapse into barbarism. In the inorganic and organic worlds he had not been taught anything, and his vast intellect was free to enter those fields and work out far-reaching principles untrammeled by early prejudices. In the ethical, political, and economic worlds he was enclosed in a shell and could grow no larger than his prison walls. To use one of those biological analogies of which he was so fond, in the physical and organic sciences he was a vertebrate with an adjustable internal skeleton, while in the moral and political sciences he was a crustacean without the power to shed his carapace.

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[1] "Life and Letters," Vol. II., p. 89.

In appraising Spencer's truly great contributions to human thought and knowledge we are therefore compelled to leave out of view all his earlier writings, except perhaps an occasional essay. His letters on the "Proper Sphere of Government" (1842), his "Social Statics" (1850), his "Principles of Psychology" (1855), his "Education" (1861), are all excluded from this high meed of praise. The same is true of Part I. of his "First Principles" (1862), which formed the stumbling-block to his whole system of philosophy, and if published at all, should have been placed at the end as a sort of appendix or curious metaphysical by-product. Solid ground is reached only in Part II. of the "First Principles," and in only one other of his works is his master mind revealed with equal clearness. His grasp here of cosmical principles is astonishing, and the vast swing of his logic carries the reader irresistibly on, sweeping majestically across the whole cosmos in many different directions, until everything is compassed in a universal scheme. Here, too, more than anywhere else, is his happy choice of expressions, never before employed, but precisely and concisely characterizing cosmical principles, singularly manifest. The word "evolution" itself, which perhaps he was the first to use in a philosophic sense,[1] though introduced in his earlier works, is here given its full meaning, and the thought it conveys has now captured the world. The phrase "redistribution of matter and motion" sums up the cosmic process as it had never been summed up by any other phrase. The assertion that evolution proceeds "from the homogeneous to the heterogeneous" has never been questioned except by those who give different meanings to the terms from those intended by Spencer, and which are the proper and even the popular meanings. Such phrases as "the instability of the homogeneous," "the rhythm of motion," "the multiplication of effects," and such single terms as "aggregation," "segregation," "equilibration," "dissolution," are all fraught with profound significance, and most of the processes described by them take

[1] "Life and Letters," Vol. II., p. 329.

place in all departments of nature. The introduction and illustration of these terms and the description of the processes of nature of which they are the names, would alone make "First Principles" an immortal work.

It is true that Spencer failed to see the essential distinction between cosmic and organic evolution, and when it was pointed out in 1877,[1] as it was not his own idea, he characteristically ignored it. He also missed the principles of creative synthesis, cosmic, organic and social synergy and sympodial development, which are quite as important as those set forth in "First Principles." But he is to be judged for what he did rather than by what he did not do. There is, however, one omission, which, deliberate and intentional though it was, has not been condoned by his readers. This is his failure to elaborate these fundamental principles of inorganic nature in a manner proportionate to that in which he elaborated the principles of biology, psychology, sociology and ethics. Uniform regret has been expressed by his readers, including his warmest admirers, that he should have abandoned this great work so auspiciously begun, and hurried on to the more special and complex sciences before laying an adequate foundation for them. The present writer was among those to express this regret and to maintain that his excuse for omitting the two volumes upon which the "Synthetic Philosophy" would and should have rested, viz., that the scheme would have been too extensive for him to complete it, and that "the interpretation of Organic Nature after the proposed method is of more immediate importance," was not a sufficient or valid excuse. It is generally felt that if these two volumes had been written, which might have borne the title of "Principles of Cosmology," it would be small matter whether the "Principles of Ethics" ever saw the light or not.

The world was even left in the dark as to how and in what order he would have treated inorganic nature had he written the omitted volumes. It is true that in the opening paragraph of the first volume of the "Principles of Sociology" he says:

> Of the three broadly-distinguished kinds of Evolution, we come now to the third. The first kind, Inorganic Evolution, which, had it been dealt with, would have occupied two volumes, one dealing with Astrogeny and the other with Geogeny, was passed over because it seemed undesirable to postpone the more important applications of the doctrine for the purpose of elaborating those less important applications which logically precede them.

The bare names, therefore, which he would have given to the two volumes were thus made known, but from them it was impossible more than to conjecture what the treatment would have been. The complete scheme was drawn up early in 1858 and sent to his father in a letter, but the revised scheme, issued in 1860, and with which we are all familiar, wants the details for all below the biology. Not until the "Autobiography" appeared in 1904 was this hiatus supplied.[1] This was the reason for publishing a letter from him dated September 19, 1895, in which considerable was said on this subject. Nearly eight years, however, were allowed to elapse before this step was taken in 1903. His permission to publish it would have been asked had it not been known that at that date Mr. Spencer was nearing his end, his death occurring in December of the same year, and it seemed highly important that information so vital to his system should not be lost.[2]

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[1] POPULAR SCIENCE MONTHLY, October, 1877, Vol. XI., pp. 672–682.

While, therefore, Mr. Spencer's treatment of inorganic nature, so far as it could be judged from "First Principles" and other indications, was full of promise, still, inasmuch as he did not fulfil that promise by an exhaustive elaboration of it, it was soon overshadowed by his work in the next great field, that of biology, the only other field of his labors in which no early preconceptions existed to warp his judgment or impede the flight of his genius.

Herbert Spencer's "Principles of Biology" is the gem of his "Synthetic Philosophy," and must rank for all time as his masterpiece. In it he founds the science of biology squarely upon that of organic chemistry, and "Chemical Development" was to have been the final topic of "The Principles of Geogeny."[3] This makes clear the filiation of the sciences thus far. Then come his several proximate definitions of life, closing with "the broadest and most complete" one: "the continuous adjustment of internal relations to external relations." Few have been satisfied with this, and the more it is studied the less it seems to fulfil the conditions. The objection to it is that there is no life in the definition. It is strange that he should have failed to cement the lowest organic science, biology, to the highest inorganic science, chemistry, by recognizing the brief step from the spontaneous molecular activities of the most complex organic compounds, the albuminoids, to the no more spontaneous molar activities (motility) of the simplest living substance, which we know as protoplasm, and believe to result from the further recompounding of the former.[4]

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[1] "Autobiography," Vol. II., p. 17; "Life and Letters," Vol. II., pp. 158–159.

[2] "Pure Sociology," pp. 66–67. A portion of this letter appears in "Life and Letters," Vol. II., pp. 90–91. Mr. Duncan should have mentioned this earlier publication of the letter in full followed by the reply to it and a further discussion of the principles involved.

[3] "Life and Letters," Vol. II., p. 159.

[4] *Cf.* "The Organic Compounds in their Relations to Life," *Proc. A. A. A. S.*, Vol. XXXI., pp. 493–494; *The American Naturalist*, Vol. XVI., December, 1882, pp. 968–979.

But this fundamental criticism aside, Spencer's handling of biological problems is nothing short of masterful. In his chapters on growth, development, function, adaptation, generation ("genesis"), heredity, variation, etc., although not a specialist in any branch of biology, he marshals an immense body of facts in support of fundamental principles, many of which had never before been discovered. In dealing with heredity he postulates the existence of "physiological units," later changed to "constitutional units." The "Principles of Biology" was published in 1864, and therefore Spencer could have known nothing of Darwin's "pangenesis," treated in his "Variations of Animals and Plants under Domestication," which appeared in 1868. But Spencer's "physiological units," as he points out,[1] are not at all the same as Darwin's "gemmules." They are still less similar to Weismann's "biophores."[2] They are nothing but "compound molecules (as much above those of albumen in complexity as those of albumen are above the simplest compounds," and are for the same organism "substantially of one kind." Why he did not admit that they are merely forms of protoplasm we do not know, but certain it is that biologists are now coming to believe that no hereditary units in the sense of independent bodies exist, and that all the phenomena of heredity, obscure and recondite as they are, can be as easily conceived to result from the action of protoplasm in various ways not yet fully understood, as from any imaginary bearers of hereditary "Anlagen."[3]

It is in Part III. on the "Evolution of Life" that the philosopher comes forth in his full power. After disposing of the special creation hypothesis, he attacks the cosmic principles underlying the organic world. Many of those enumerated in "First Principles" are shown to be in full force on the biotic plane. The process from homogeneity to heterogeneity finds its clearest exemplifications here, and the two great principles of differentiation and integration are formulated and illustrated with wonderful force. We can not here enumerate all the biological principles set forth in this work, but the application of the principle of equilibration to the organic world can not be passed over in silence. The Lamarckian principle of increase by use and atrophy from disuse, called somewhere by Spencer "use-inheritance," and early recognized by him as "the inheritance of functionally-acquired modifications," now becomes, in the new terminology of biology, "direct equilibration," while natural selection, which Spencer, along with many others mentioned by Darwin in the later editions of the "Origin of Species," had foreshadowed before that work appeared, becomes "indirect equilibration." The discussion of these two principles is among the most profound of all of Spencer's writings. The subject, so intimately connected with this, of the transmission of acquired characters, was not overlooked in the "Principles of Biology," but it was not brought into the foreground until Weismann's "Essays," began to appear, denying its possibility. Spencer, as is known, entered the lists with his paper on "The Factors of Organic Evolution," and continued to reply to Weismann for a number of years. In him the trained biological specialist found a foeman worthy of his steel. Readers of these papers on both sides will of course differ in their judgments on the argument according to their cast of mind, but all will admit that Spencer's presentation of the case was able, and to it, as much as to anything else, were due the many notable concessions that Weismann was from time to time compelled to make.[1]

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[1] "Life and Letters," Vol. I., p. 199.

[2] *Ibid.*, Vol. II., p. 52.

[3] *Cf.* Minot, "The Problem of Age, Growth, and Death," New York, 1908, pp. 233 ff.

In the second volume of the "Principles of Biology," devoted mainly to morphology and physiology, Spencer showed that he could play the rôle of a specialist, but his special studies and illustrations all have a philosophic purpose in establishing principles. These, however, belong for the most part to the minor or more special laws of biology, and do not call out the same philosophic powers as the major and more general laws dealt with in the first volume. Perhaps the most important of these laws is what he calls the "antagonism between growth and sexual genesis," which might otherwise be stated as the law that nutrition and reproduction are inversely proportional. The truth of this is known to practical breeders, florists, and horticulturists, but not to the general public, and it has some interesting results.

Spencer lived to revise his "Biology" and introduce into it much of the Weismann controversy and other features which had not presented themselves clearly at the time the work was originally written. Upon the whole it is a remarkable work. Surprise has often been expressed that trained specialists in biology had rarely or never been able to trip him on any of his statements. This is partly explainable by the fact that Professor Huxley read the proof on a considerable part of the work, but it does not appear that he found much to correct, and we must admit that Spencer possessed a remarkable faculty of accurately stating biological facts that he had not himself observed, and a still greater talent for correlating and interpreting them and fitting them into his universal scheme.

Let us now turn to the "Principles of Psychology." In his "Synthetic Philosophy" Spencer placed it after the Principles of Biology. Although he says little about his reasons for this arrangement, it seems clear that he regarded it as the order of evolution. Yet in his attacks on Comte's serial classification of the sciences he denies that there is any serial order.[1] But whenever he mentions the subjects of his "Synthetic Philosophy" he always arranges them in the same order,[2] corresponding to that of his original program and of all subsequent programs. No one will probably question the propriety of this arrangement, and it may be inferred that he regarded psychology as having some such relation to biology as the latter has to chemistry, *i. e.*, as in a sense growing out of it. Now, whereas he does clearly show this filiation of biology and chemistry, it is difficult to find in his psychology a recognition of its dependence upon biology in the same sense. This is probably due to the fact that the "Psychology" was first written and published as an independent work several years before he conceived the idea of a "Synthetic Philosophy," and afterward revised, enlarged, and adapted, and then set up in its proper niche in the general structure. But the task of adapting it was not an easy one, and he seems to have devoted himself more to what he regarded as its improvement, to the answering of criticisms, and to bringing it up to date, than to linking it on to his "Biology," which stands before it, and to his "Sociology," which was to follow.

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[1] *Cf.* Weismann's "Concessions," POPULAR SCIENCE MONTHLY, Vol. XLV., June, 1894, pp. 175–184.

He wrote the "Psychology" when fresh from the reading of Hamilton, Mansel, Mill, and Kant, and the point of view was that of the old philosophy of mind, which he, indeed, attacked, but scarcely from the modern scientific point of view. This is more true of the second volume than of the first. The work opens, as do most works on psychology, with a treatise on the nervous system, and the chapter on Æstho-physiology is certainly luminous and forms a new departure. His definition of mind as consisting of feelings and the relations between feelings is inexpugnable. In part III. he treats of life and mind as "correspondence," but does not seem to regard mind as an outgrowth of life. In treating pleasures and pains at the end of part II, he recognizes the existence of feelings which do not consist of pleasure or pain, and even calls them "indifferent," but he does not there or elsewhere show that the function of such feelings is to furnish knowledge. This was perceived by Reid, though he did not grasp its import. Spencer thus fails to show the genesis of the rational powers. He clearly sets forth the biologic origin of feeling, but he does not perceive that the intellect was also an advantageous attribute whose origin can be explained on natural principles. Notwithstanding the acknowledged ability of this work, these and other deficiencies deprive it of the title given it by some of being Mr. Spencer's *chef d'œuvre*. Standing as it does between the "Biology" and the "Sociology," with neither of which it is adequately linked, it seems isolated and solitary. The failure clearly to affiliate mind upon life is not its worst fault. From the standpoint of the sociologist the most glaring defect is the absence of all recognition of the psychologic basis of social phenomena. Neither in the "Psychology" nor in the "Sociology" which follows is there to be found any attempt to show what are the underlying causes of social phenomena. The nature of social energy which moves the world is nowhere set forth, the distinct rôles played by feeling and thought, as the motor and rector[1] agencies of both the individual and society are not recognized, and both psychology and sociology are thus reduced to mere descriptive sciences. Much the same may be said of his failure to recognize a vital energy in biology with motility as the dynamic agent, which also leaves biology in the descriptive stage. Life and mind are forces, and organic, psychic, and social structures are magazines of energy. Any system that fails to recognize this is not a full-fledged science.

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[1] "Life and Letters," Vol. I., p. 97; "Pure Sociology," p. 66 (this part of his letter is omitted in the "Life and Letters," Vol. II., p. 90).

[2] "Life and Letters," Vol. II., pp. 285, 328.

It may be said that Spencer constantly insisted that it was feeling and not ideas that moved the world, as opposed to Comte's statement that ideas govern or overthrow the world. It is clear that he misunderstood Comte, who held the same view as Spencer, and that the two statements are not antagonistic.[2] Spencer also said that "the will is a product of predominant desires to which the reason serves merely as an eye."[3] This is very true, and Schopenhauer had said it forty years before him. But such scintillations of the truth do not make a science nor justify us in saying that he thereby furnished sociology with a psychologic basis.

Coming now to the "Principles of Sociology," we find that the work was not hampered by any previous work, and, as in the "Biology," the field was clear for a new start in a most alluring direction. If the order in which the volumes of the "Synthetic Philosophy" stand is the order of nature, marking the course of evolution, we should expect to find the "Sociology" opening with a chapter or an introductory part setting forth the causal connection between sociology and psychology. But, just as no causal connection was shown between biology and psychology, so none appears binding psychology and sociology together. This confirms what was said of the isolated condition of the "Principles of Psychology." What we do find, however, is a rather definite intimation that it is biology rather than psychology that forms the natural basis of sociology. How could anyone be expected to doubt this when nothing is said in the first volume of the "Sociology" about its relation to psychology, while, after the long treatise on the beliefs, customs, and ideas of

[1] Used by Fourier in this sense ("*moteur et recteur*").

[2] *Cf.* "Applied Sociology," pp. 41–43.

[3] *Westminster Review*, January 1, 1860, p. 93.

primitive races, belonging rather to anthropology, we find in part II. that "a society is an organism," and that social growth, social structures, social functions, and social organs are treated from the strictly biological point of view? Mr. Spencer denied that he based sociology upon biology and censured two American authors for intimating that he seemed to do so, but the comparison that he used is not at all apposite.[1]

The other two volumes of the "Principles of Sociology," based as they are on his great compilation, "Descriptive Sociology," are above criticism in their comprehensive sweep as a vast induction. Some of his facts will, of course, be denied, but he admitted that the reports of travelers must be taken with many grains of allowance. Yet these are almost the only sources from which an author who is not himself a traveler must rely. For those therefore who consider such a work to constitute "Sociology" the only vulnerable part is the terminology, classification, and arrangement of the subject-matter. The phrase "ecclesiastical institutions" may be justly objected to as seeming to predicate something like a church of the religious structures of primitive man. The word "ecclesiastical" might be stretched sufficiently to justify this were there no better term, but it is universally admitted that the priesthood was practically coeval with human society, and we possess an adjective corresponding to this noun which is more euphonious and more expressive than the one used. By all means, then, should the phrase *sacerdotal institutions* be substituted for "ecclesiastical institutions." The introduction of "political institutions" between the "ceremonial" and the sacerdotal is a forced arrangement. The ceremonial are largely sacerdotal, and their separation is difficult. The sacerdotal should probably stand first, and the "professional," beginning with the "medicine man," so similar to a priest, should follow. "Political institutions" would then be in order, to be followed by "industrial institutions." But Mr. Spencer had no conception of gentile society and the fundamental distinction between it and political society, so clearly set forth by Morgan. This classification shows how late the latter class of institutions have always been in the historical development of society. Still less was he acquainted with that other most important of all transformations which is undergone by every advanced society at the proper stage in its history, viz., union and amalgamation of groups, whether through war or peace, by which a third and higher group results from the blending of two lower groups, constituting what is appropriately called the cross-fertilization of cultures. It is only through this that all the higher political, industrial, economic, and professional institutions arise.

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[1] "Life and Letters," Vol. II., p. 357.

After thus threading the mazes of cosmic, organic, psychic, and social phenomena, we come at last to the "Principles of Ethics," which Mr. Spencer regarded as the crown of his system. One can not but be struck by the resemblance in this respect of Herbert Spencer's career to that of Auguste Comte. Both began with a lively interest in what may be called political ethics, an interest which they both continued to feel through life. But both saw, after their early survey of the field, that the world was not ready for their final achievement, and therefore both stopped and devoted twelve or fifteen years of arduous labor to laying a scientific foundation for the *magnum opus* which was to reform the world. Comte laid special stress on this, and placed as a motto at the head of the first volume of his "Politique Positive" the lines of Alfred de Vigny:

> Qu'est-ce qu'une grande vie?
> Une pensée de la jeunesse, exécutée par l'âge mûr.

Spencer could not even wait to complete the last of the preparatory works, and stopped in the middle of it to write the final work. So strongly was he impressed by the importance of this last work, and so apprehensive that he might not live to complete it, that he said in the preface to the first part ("Data of Ethics" issued separately):

> I am the more anxious to indicate in outline, if I can not complete, this final work, because the establishment of rules of right conduct on a scientific basis is a pressing need. Now that moral injunctions are losing the authority given by their supposed sacred origin, the secularization of morals is becoming imperative. Few things can happen more disastrous than the decay and death of a regulative system no longer fit, before another and fitter regulative system has grown up to replace it.

The implication of course is that Herbert Spencer's "Principles of Ethics" will henceforth constitute the Koran of moral doctrine to the exclusion of all other codes! Comte has been pronounced an egotist and a fanatic for proclaiming himself the high priest of the religion of humanity, but he never assumed to be an infallible pope in the domain of moral conduct. The parallelism, however, does not end here. The world has passed judgment upon Comte's career, and while his final work for which he had lived and labored, viz., his "Positive Polity," has been declared a mistaken dream, the preparatory work, his "Positive Philosophy," which he intended to be only the pedestal upon which the monument was to stand, is looked upon by most men as a path-breaking, by many as an epoch-making achievement, and as marking the beginning of scientific philosophy. In Spencer's case it is too early to speak thus definitely, but all things point to the complete rejection of his political ethics as outlined in "Social Statics" and perfected in his "Principles of Ethics" and "Man

Versus the State," while his cosmic philosophy, which he regarded as little more than a foundation for the other, grows more solid with time, and is clearly seen to be too massive for the flimsy superstructure that he sought to erect upon it.

In Spencer's system ethics is placed last in the series of subjects or sciences, as if it were the highest evolutionary product. Although he laid no stress on the serial arrangement of the sciences, and in his little book on the "Classification of the Sciences," written to refute Comte's "hierarchy," he practically ignores it, still he could not answer the charge of arranging his volumes in practically the same order as Comte arranged the sciences. Nor did he deny that he regarded this as the order of evolution. As Comte did the same for his "Morale," the implication is that they both regarded ethics as a science of the same type as the others, only higher in the scale, and, in fact, the highest of them all. It would thus grow out of and be affiliated upon sociology. The treatment of it does not in either case sustain this claim. In Spencer's case this is much more marked than in Comte's, because they had quite different ideas of what constitutes a moral science. Spencer, as we have seen, regarded it simply as a "regulative system," which is not a science at all. His treatment of it virtually carries out this idea, and his "Principles of Ethics" scarcely differs from the traditional moral teaching of other writers, except that it is "secular" and recognizes no religious or "ultra-rational" sanction. After he had abandoned his "absolute ethics," as set forth in his "Social Statics" (expunged from the last edition), but shown to be false by a study of the widely divergent moral ideas of the races of men, the last claim to the title of a science had been withdrawn from ethics, and it stood at the head of the system having no organic connection with the other sciences of that system. What, then, is his ethics? It is simply an attempt to make a practical application of the true sciences, especially of sociology, to human needs. In so far as it is a science in any sense, it is an applied science, and the greater part of it may be denominated applied sociology.[1]

The subject of Spencer's relations with Comte and the similarity of their ideas has been purposely avoided in this article, because it presents the least attractive side of a great man's mind. His overweening affection for what he called "the progeny of the brain,"[2] his intense love of originality, which often seems to exceed his love of truth, and his morbid sensitiveness to any apparent appropriation of his ideas, blinded him to the merits of others and often led him to refine upon a distinction without a difference. Jealousy as well as envy may be a compliment. This can alone explain Spencer's attitude toward Comte. He must have felt, and been oppressed by the fact that there was a man on the other side of the Channel who, like himself, was striving to give the world a philosophy of science. And in fact, from this point of view, Auguste Comte was to the first half of the nineteenth century what Herbert Spencer was to the second half.

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[1] *Cf.* "Applied Sociology," pp. 317–318.

[2] "Life and Letters," Vol. I., 254.

A fuller discussion of the merits and demerits of Spencer's political ethics is also purposely avoided in this place, partly for want of space, partly because it would have to be too critical for the appraisement here proposed, and partly because it has already been attempted, not only by others but by the present writer.[1]

The statement has been so frequently made that Mr. Spencer's sociology and his political and economic doctrines do not logically follow from his law of cosmic and organic evolution, that something more might naturally be expected here on that point than has been said above as incidental to the discussion of other questions. This subject is, however, too large to be treated here, and would require a separate article. This lack has been partially supplied in other places and to these sources the reader is referred.[2]

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[1] "The Political Ethics of Herbert Spencer," *Annals of the American Academy of Political and Social Science*, Vol. IV., January, 1894, pp. 582–619. Publications of the Academy, No. 111.

[2] "Herbert Spencer's Sociology," *The Independent*, New York, March 31, 1904, Vol. LVI., pp. 730–734; "Herbert Spencer's Autobiography," *Science*, N. S., June 10, 1904, Vol. XIX., pp. 873–879; "The Sociology of Political Parties," *American Journal of Sociology*, January, 1908, Vol. XIII., pp. 439–454.

January, 1909—Ætat. 67.

546. The Status of Sociology

History.—Written November 8–11, 1908. In July 1908, I received a letter dated Zurich, June 29, from A. Eleutheropulos, whose little work on *Soziologie*, 1904, one of the series entitled *Natur und Staat*, I had known since it appeared, giving a detailed account of the new sociological journal that he was about to start with the financial coöperation of Baron v. Engelherdt. It was to be called the *Monatsschrift für Soziologie*, and he asked my "Mitarbeiterschaft" in the enterprise. Baron Engelherdt's name was also attached to the letter, which was scarcely more than a circular addressed to sociologists. I replied offering to do anything in my power and asking to be considered a subscriber from the first number. A printed circular was sent out stating that the first number would be devoted to a discussion of the "Aufgabe der Soziologie," and soliciting contributions from leading sociologists. On September 24, I wrote offering to send a short article on that subject. The articles were to be in any of the principal languages, each writer to write in his own language. On October 14, I received a postal card from Eleutheropulos saying that he would be glad of my contribution. In my rough draft I used the word "Province" as translating the German "Aufgabe," but I subsequently changed it to "Status," as more nearly describing my treatment of the question. I sent the article on November 12. No proof was sent to me, and on January 25, 1909, I received a letter saying that No. 1 had appeared and contained my article, and that copies and reprints had been sent me. They reached me on February 3. My middle initial was printed "J" instead of "F," all through and over the pages, and there were seventeen other typographical errors, some of them abominable and absurd. The German *résumé* of my article by Eleutheropulos was pure nonsense.

His idea of how to conduct a magazine seemed to be to make others do the work, and he had sent me several rather peremptory requests to send him certain matter relating to American sociology. I had referred them to persons who I thought might possibly help him, but I think none of them could do so without turning aside from their work to do purely clerical service for him. There was to be a trifling honorarium for the articles, but he wrote me that it would have to be waived. The first number was so stupidly done that I hoped subsequent ones might show an improvement. As I had subscribed and was ready to pay whenever I should receive a bill or notice of the amount due, I waited to receive the numbers as they appeared, but none came, and I began to think the enterprise was stillborn, and had died with the first number. But I afterwards learned that it had been going on and the numbers were not being sent to me. I therefore wrote to know the reason why I did not receive them, and I got an insulting postal card saying that I had not sent my subscription and they did not send the *Monatsschrift gratis*. My name was printed on the cover along with others as one of the *Mitarbeiter*. I immediately wrote and ordered it removed, and informed him that I did not wish to receive any further communications from him. I have since learned that the enterprise soon died, as of course it must in such hands.

This article is the nearest approach to a complete but enormously condensed summary of the whole science of sociology that I have ever made. Almost every sentence states a principle susceptible of any amount of expansion. It might be made a basis or syllabus of several courses of lectures. It was reprinted entire in the *Plebs* Magazine for November and December, 1910.

Monatsschrift für Soziologie, Zürich, I. Jahrgang, Heft 1, Januar, 1909, pp. 30–40; The Plebs Magazine, Oxford, Vol. II, No. 10, November, 1910, pp. 231–235; No. 11, December, 1910, pp. 255–258.

547. Review of "The Problem of Age, Growth, and Death. A Study of Cytomorphosis, based on Lectures at the Lowell Institute, March, 1907." By Charles S. Minot. New York and London. G. P. Putnam's Sons, 1908. Pp. xxii, 280. The Science Series

History.—I received the book from Mr. F. W. Hodge on October 15, 1908, finished reading it on the 24th, wrote the rough pencil draft on the 26th, and the final draft on the 31st, read the proof on December 21, and the number containing it reached me on February 9, 1909. In reading it I detected an error in line 3 on page 669, where "inorganic" was printed for "organic." As the copy was not returned with the proof, I was curious to know how I had written it in the final draft, it being correct in my rough pencil draft. Mr. Hodge returned my manuscript, and it proved to be my own mistake in rewriting it. I could only correct my reprints. A still worse error was made in the last paragraph where I inadvertently wrote Verneuil for Chevreul. It was a pure aberration of mind.

The American Anthropologist, Lancaster, Pennsylvania, New Series, Vol. X, No. 4, October–December, 1908, pp. 668–669.

548. [Notice of the Seventh Congress of the International Institute of Sociology]

History.—Written May 9, 1909. On the 8th I had received from Dr. Worms a notice of the Congress with the request that I have it published in some prominent place, and I sent it to the Economic Bulletin, where it appeared in the June number (Vol. II, No. 2, pp. 111–112). At the same time I sent this shorter notice to *Science*.

Science, New Series, New York, Vol. XXIX, No. 751, May 21, 1909, p. 811.

The seventh Congress of the International Institute of Sociology will meet in Bern, from July 20 to 24 next at the invitation of the Swiss Government. The sessions will be held in the university and under its auspices. Senator Baron R. Garofalo, of Venice, will preside, and the subject of social solidarity will be discussed from a variety of wholly scientific points of view. Representatives of many countries, including the United States, will be in attendance. It will be remembered that the last congress was held in July, 1906, at the University of London, under the presidency of the eminent French economist, M. Levasseur, when the opposite question of social struggles was exhaustively canvassed.

October 16, 1909—Ætat. 68.

549. The Geneva Jubilee

History.—Written September 18, 1909. On September 7, Mr. H. L. Koopman had asked me to write something on this subject for the *Brown Alumni Monthly*, and I promised to do so provided it should appear anonymously. I did not wish to appear in the light of prematurely boasting of my connection with the affair, although I did regard it as a great honor. Then again, as I was the senior delegate, I expected to be called upon to make a report on it to the faculty, and did not wish to forestall that report. I met Mr. Koopman soon after, and he said he must have it at once if it went into the October number, so on September 18, I sat down for an hour and dashed this off in pencil to submit to him for corrections and suggestions. I could not find him that day and left it in his office with a note to that effect. Instead of bringing it to me to talk over so that I could remodel it and make a final draft, he sent the pencil sketch at once to press. Proof came on the 30th with my name over the article, and I struck it out. I also insisted that the French accents be supplied, but they said there were none in the font. On the 16th I received a request from President Faunce to present my report on the Jubilee the Tuesday following. I always have a graduate class of teachers on Tuesday afternoons at the same hour as the faculty meeting, which prevents my attending them. I saw Professor Greene and arranged with him to make the report, lending him certain documents that he did not have, and wrote to the President explaining the matter. Professor Greene made a very satisfactory report.

The Brown Alumni Monthly, Providence, Vol. X, No. 3, October, 1909, pp. 56–57.

BROWN UNIVERSITY REPRESENTED AT THE IMPRESSIVE CEREMONIES

IT is well known that the University of Geneva celebrated this year (July 7–10) the 350th anniversary of the founding of the "college" or "academy" by Calvin out of which the university has grown. The various universities and other institutions of the world were invited to cooperate by sending delegates. Very many complied, and the United States was not backward in the matter. Other universities will doubtless publish reports from their delegates. We are concerned only with Brown University, which responded to the invitation by sending two delegates, viz., Professors John F. Greene and Lester F. Ward. They were provided with proper credentials in the form of a parchment roll in which were expressed in choice Latin the sentiments which Brown University entertains toward the University of Geneva.

In this document, which was duly presented by the delegates, the president and faculty of Brown University, after thanking the rector and senate of the University of Geneva for the invitation to join in the jubilee festivities, point out that all institutions of learning are held together by common bonds, and that as Brown University and the University of Geneva are especially so united by one and the same love of liberty, the former, though its history is much shorter, is moved by the same sentiments, and feels that it is separated rather in space than in spirit. And they add:

"We congratulate you heartily on having given to the world so many men learned in letters and in science and distinguished in the church and in the forum; and we congratulate you also that after the darkness of ages your light not only began at last to shine, but that more and more it has continued to illuminate the minds of men."

The programme of exercises was elaborate and it was carried out to the letter. On the first day (July 7) the delegates and invited guests were installed and duly lodged at the expense of the authorities in the various hotels of the city, and in the evening a reception was given them by the rector and senate of the university at the National Hotel. The solemn séance of the opening of the jubilee took place on Thursday morning, July 8, the delegates meeting at the university and marching in a column to the Cathedral of St. Pierre, where the exercises were held. There were appropriate addresses by prominent public men, including the president of the Swiss Confederation, and these were followed by the call of delegates in the alphabetical order of countries, Germany (Allemagne) heading the list, followed by the United States, which was classed as "Amerique." The universities and other institutions were called upon, not by their names, but in the alphabetical order of the cities in which they are located. Because of the great number and the shortness of time, the addresses of the delegates were few and brief. Dr. Schurman presented the case in a few words for all the American universities.

The second solemn séance was held on Friday morning, July 9, in Victoria Hall, at which, after addresses by the rector, M. R. Chodat, by M. Charles Borgeaud, the historian of the university, and others, the honorary degrees conferred were announced by the heads of the several departments. These have since been published and are generally known.

There was a grand historical "cortège" on Saturday, July 10, for which the most elaborate preparations had been made, the costumes, we learn, having been, in part at least, brought from Berlin for the occasion. They were certainly bizarre and on a large scale. The line of march was set forth in the programme, starting at 5 and returning at 7:30, and concluding with a "fête de nuit" lasting until 10. This was followed by a "grand commers," organized by the students and held in the Palais Electoral. It was a sort of carnival with a great variety of entertainments, and continued far into the morning hours, the advent of Sunday having no effect in checking the hilarity.

It should be said that almost incessant rain during the entire four days of the jubilee tended to put a sort of "damper" on the whole proceedings, especially the outdoor exercises, but it did not prevent the carrying out of the programme, and even the great "cortège" plodded its way along in the rain and the muddy streets.

Besides the more essential part of the affair, as above described, there were various pleasant side-entertainments of a social character, which were to many the most enjoyable part. Especially to be mentioned are the two dinners or banquets, the first given on Thursday evening at the National Hotel by the Council of State of the republic and the canton of Geneva to the delegates, and the second, given on Friday evening at the Palais Electoral by the university, to the delegates and invited guests. These were noteworthy events, attended with the usual prolonged postprandial speech-making.

On Thursday afternoon M. Casimir de Candolle, secretary of the Société Académique, gave a garden party at Vallon in the country, but many were prevented by the rain from going. The name de Candolle is the most illustrious one in the scientific history of Geneva. Casimir de Candolle represents the third generation of that famous line of botanists. It was his grandfather, Auguste Pyrame, who began, and his father, Alphonse, who completed the great Prodromus in seventeen volumes, in which every phanerogamous species of plant was described in Latin. So important was the completion of this work regarded that the Pope ordered a medal to be struck in its honor. Casimir de Candolle and his son, who is the fourth in the line, are continuing the botanical work thus begun by publishing supplementary monographs to keep abreast of the march of the science. The curious thing connected with all this is the fact that this work has not been done at the University of Geneva, and that the wonderful herbarium of the de Candolle family is, and always has been, in buildings in the Cour St. Pierre, No. 3, in front of the cathedral.

Another pleasant affair was the boat excursion on the lake and tea at the Creux de Genthod on the lawn of the old home of so many men of science, given on Friday afternoon by M. and Mme. de Saussure who are now its proprietors.

The delegates received quite a flood of literature descriptive of the event and of the city of Geneva and its surrounding attractions, guide books, etc., and including an extensive extract in pamphlet form from Borgeaud's Histoire de l'Université de Genève, a prospectus of that work, and a bound folio volume consisting of that portion of it which relates to the part played by Napoleon in realizing the present university, which, as Borgeaud shows, was considerable, and justifies his expression: "Université de Napoléon."

550. Ludwig Gumplowicz

History.—Written October 4–5, 1909. On September 29, I received a Separatabdruck from the Wiener Wochenschrift "Die Wage" of a 4-page article by J. K. Kochanowski entitled: *Ludwig Gumplowicz*, and dated Warsaw, 26 August. It was my first intimation of his taking-off. I sent it to Dr. Small and asked him if he did not think it ought to be translated and published in the *Journal*. He replied under date of October 2, that Mrs. Unger had also sent it to him and he had asked her to translate it, adding that he would be glad to have something also from me, and saying:

> "As you are the only man in the United States who knew Gumplowicz intimately, as for other reasons, it would be most appropriate that some reference to him from you should appear in the *Journal.*"

Meantime (October 1) I had received a letter from Mrs. Unger enclosing her last letter from Gumplowicz and another clipping from some German paper announcing the suicide of the two unfortunates and stating that the poison taken was cyanide of potassium. I then wrote this little obituary and sent it to Dr. Small on the 5th. On November 22, I received the little posthumous work, *Sozialphilosophie im Umriss*, from his son Dr. Wladyslaw Gumplowicz in Vienna, which the author had mentioned in his letter (see infra, No. 553). The number of the *Journal* containing the paper reached me December 3, and some separates on the 21st. I sent out the latter to all who I imagined would be interested.

There is nothing on which people differ more than on the ethics of suicide. By most it is regarded as a crime, and Sir Peter Laurie proposed to "put it down." That pure pessimism does not encourage it is shown by the fact that Schopenhauer condemned it. Still Nietzche regarded it as evincing great heroism. One of these leaflets fell under the eye of Dr. W. H. P. Faunce, President of Brown University, and he took the trouble to write me a letter (dated December 23, 1909), which is very sympathetic but contains the following remark on the subject we are considering:

> "The only point on which I might differ with you would be in your apparent judgment regarding the manner of his ending. When a man says regarding a surgical operation, 'I can not reconcile myself to it, and am resolved to take my life in case the opinion of the doctors should be found correct,' I should say that he had acquired very little courage from seventy years of rich experience."

This view seems to be a remnant of the general opposition of Christianity to suicide, and yet we are now coming to regard martyrdom, which Christianity so highly extols, as a form of suicide. This article was reprinted entire in the *Plebs Magazine.*

The American Journal of Sociology, Chicago, Vol. XV, No. 3, November, 1909, pp. 410–413; The Plebs Magazine, Oxford, Vol. I, No. 12, January, 1910, pp. 268–270.

A GREAT sociological light was extinguished when, on August 20 last, Ludwig Gumplowicz ceased to exist. The double suicide of this remarkable man and his accomplished wife, Franciska, is now well known to the world. It was a deliberate act on the part of both, without any brain-softening or the slightest tinge of an unbalanced mental state on the part of either. They were a devoted couple, and his prolonged sacrifice for her during so many years of her invalid life would form the subject for a romance of heroism, could it be written. Life for either without the other was impossible, and the dread malady would soon, as both knew, have left her alone. Simultaneous death was far preferable. Nietzsche says that the suicide displays such heroism that he almost deserves to live. Never was this more true than in the present case.

As one of the certainly very few Americans who knew Gumplowicz personally, it seems to me to be almost a duty to join in the general expression of regret at his loss. But his work was certainly done, and his tragic end only prevented a far sadder one by a few months at most. He had passed his seventy-first year, and even good health would not have preserved him for science many more active years. There is, therefore, reason, for being reconciled to the fact as it has transpired.

The readers of this *Journal* know[1] that I took special pains to journey from Vienna to Graz in 1903 on purpose to meet him and thresh out face to face some of the problems that we had long been discussing in letters. He has been called a pessimist, and perhaps deserved the name, but one passage in the article of his to which I have just referred, well describes himself; and never will it be more appropriate to repeat it than now. He says:

> When we speak of "pessimists," we think of morose growlers who are always cursing and never satisfied; while we mean by "optimists" people who are content with themselves and the world. This association of ideas is far from exact. On the contrary, the facts are precisely the reverse. The pessimist in world-philosophy is usually an optimist in life. The troublous course of the world does not surprise him; he expects nothing better; he knows that the world is evil, that it cannot be otherwise. Thus he has no ground whatever for being unsatisfied with life; it is as it always was, and always will be. He enjoys *des Lebens Unverstand* as a comedy of nature. The case is different with the optimist in world-philosophy. Convinced that things may be better if man will only better himself, he stumbles against rude disappointments at every step, and he is constantly complaining that men, and with them life itself, are ever failing to make improvement. In eternal expectation of better times, he experiences constantly new disappointments and falls from one despair into another. The optimist in world-philosophy usually presents to us in life the picture called up by the word "pessimist."[2]

Conformably to this general description I found him the gentlest of men, the type of kindly suavity and apparent contentment with his lot, which was hard even then, as he was absolutely confined to Graz and to the tender care of his beloved invalid wife. As to the latter, I was unable to see her on that occasion, as her health did not permit, but three years later, when I again visited Graz and enjoyed a second prolonged interview with Gumplowicz, I was so fortunate as to meet her a few minutes in the park and converse with her. She was a highly cultivated and refined lady with a well-stored mind and charming manners, speaking French without the slightest accent. I could well understand his devotion to such a person.

It was he who had prevailed upon the Wagner Publishing House in Innsbruck to publish Mrs. Johanna Odenwald-Unger's German translation of my *Pure Sociology*, and had also volunteered to correct the proofs. I visited Innsbruck in 1906 while the work was in press, and went thence to Graz. He saw the work through and in his many letters evinced a profound interest in my views, though often so much opposed to his own.

Toward the end of 1907 his health began to fail, and in a letter dated November 15, 1907, he mentions it and says: "The doctor describes my condition as 'nervous prostration in consequence of a nervous shock,'" but he did not intimate at that time anything more definite, although it seems that a council of doctors had pronounced his case one of cancer of the tongue. In another letter dated February 1, 1908, he writes:

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[1] See the issue of March, 1905, p. 645.

[2] *Ibid.*, pp. 643, 644.

> My seventieth year is giving me some very unpleasant experiences, but I am not sick, and moreover I am a perfectly free man. . . . I have promised myself now to make up something of what I have been neglecting during the last ten years in the domain of sociology—and therefore, first of all, to occupy myself with your sociology (which is really a cosmology) is something that I greatly desired. The correction of the proofs is a very agreeable (and now almost my only) occupation.

It was in his letter of October 24, 1908, that he first explained to me the nature of the "nervous shock" of a year previous. In that letter he says:

> Things are going better with me now, and they were really bad only in the opinion of the doctors, who appear to have been mistaken. This is the way it was: At the end of 1907, in consequence of a wound made by the sharp corner of a tooth there was formed a small scar and swelling on the tongue. The physicians suspected a *carcinoma* and advised an operation. I could not reconcile myself to it, and was resolved to take my life in case the opinion of the doctors should be proved correct. However, such was not the case. Since the vote of the doctors ten months have elapsed and no single symptom of *carcinoma* has shown itself—on the contrary the swelling has diminished, and I find myself quite well.

Alas! it was he who was mistaken.

My last letter from him is dated March 28, 1909. *Pure Sociology* was out (the German edition), and he had offered to revise the manuscript of Mrs. Unger's German translation of *Applied Sociology*, not yet finished. At the end of the letter he says:

> I regret that I shall not survive the appearance of your *Angewandte Soziologie*, for I am in a bad way—my wife also, who thanks you heartily for your friendly words, is very poorly, so that we are both thinking more of the other side (*an's Jenseits denken*), and life is a burden to us. I am sorry that I must close this letter with such bad news about myself, but I do not consider my situation as at all tragic, for I have already reached my seventy-second year. I have also the satisfaction of seeing my *Rassenkampf* appear in the second edition (after 26 years!), and before my departure I shall leave to the world still another little *Sozialphilosophie*—my swan song.

The new *Rassenkampf* with all his early papers appended, in which he first set forth his great theory in 1875, reached me on May 27. This work, as he told me at the beginning of our correspondence contains his "system." His other numerous works are only amplifications of it. No one understands Gumplowicz who has not read his *Rassenkampf*. The *Schwanengesang* I have not yet seen. I acknowledged the former with hearty congratulations, but I was then just leaving for Europe, and I received no further word from him. Mrs. Unger, however, received a short pathetic note from him dated August 8 (twelve days before the fatal act) which she has kindly allowed me to see. He tells her that it will be his last letter to her and that he writes it with great effort. He makes a suggestion regarding the English translation of the *Rassenkampf* (undertaken by Mrs. Unger some time ago from the original edition, but not completed), sends his greetings to her and, in a postscript, to me.

At the banquet of the Bern Congress of the International Institute of Sociology on August 21, a postal card to him was circulated and packed with signatures of admiring friends. I made mine so plain that he could not overlook it.

Thus has passed from our midst a striking figure, which, whatever may be the fate of his theories and original ideas, will ever stand as a prominent landmark in the history of sociology. The heaviest obligation under which sociologists now rest, and the one which is most neglected, is that of recognizing, appreciating, and utilizing the work of their fellow-sociologists, and that of Ludwig Gumplowicz deserves this honor as fully as any other work done in this field.

November–December, 1909—Ætat. 68.

551. Education and Progress

History.—Address delivered extemporaneously August 2, 1909. Stenographic report revised August 20–21. In 1909, the conservative element acquired control of Ruskin College, Oxford, which, under the able direction of Mr. Dennis Hird, had been teaching evolution, sociology, and science generally to the working classes, and it was necessary to found a new college that should be under the full control of the labor organizations. Such was the Central Labour College, to which most of the Ruskin College students at once stampeded. Its foundation was to be ratified in a great labor educational convention to be held in Oxford on August 2, 1909. On May 28, I received a letter from Mr. Hird treating of various subjects which had been discussed by us, and concluding as follows:

> "Now, far more important than all, can you be in Oxford on August 2, our bank holiday? . . . There is a great meeting of old and present Ruskin students on the 2d, and I can promise you the finest reception you ever received."

I replied on May 31, that I was coming to Europe by the southern route and would return from Liverpool about August 7, which would naturally enable me to be in Oxford on August 2, if nothing interfered with present plans. I asked him to write to me at Bern on July 20. This for some reason he failed to do, but when I reached London on July 29, I found very urgent letters from him and I telegraphed him that I would be in Oxford the next day. He was at his home in Bletchley, but a number of his friends met me at the station. The program of the ratification meeting was in my London mail on which I was down to address the convention. I certainly was given a great reception and met many of the students who had been studying my books and seemed delighted to see me. In fact I was so constantly entertained that I did not get a moment to prepare my address, and concluded not to attempt any preparation, but to drift with the tide and be impelled by the influences of the occasion altogether. This in fact is what I did do. I attended the business meetings during the day and was intensely interested in the calm, cool, and intelligent manner in which they were conducted. Delegates from labor organizations in all parts of the kingdom were present, men, who, I was told, worked regularly at their trades, some of them miners, and many of the speeches made were really eloquent. After all the business was transacted they called on me in the morning session, but I spoke only a few minutes then, promising to speak in the evening. It was in the evening that this address was delivered. A stenographer was present and took it down verbatim I think. There was great applause at the close, and all rose in a body and sang "For he's a jolly good fellow, etc." Mr. Hird then spoke and there was the greatest good feeling. I stayed over a day at their urgent request on purpose to enable the delegates and students to meet me at the Central Labour College, and there I had a second ovation. They catechized me sharply and I was surprised at the kind of questions they asked. They were not about any personal matters, hours of labor, wages, etc., but about the constitution of the universe, the origin of the solar system, of the earth, of life on the globe, and of the human race. Such are the questions that present themselves to the laboring classes as soon as they begin to receive a little real education.

Among the many delightful people whom I met during that brief sojourn in Oxford, and whom, if this were the place, I would be glad to mention, was Mrs. Montefiore, whose fame had gone before on account of her refusal to pay taxes because not represented, and submitting to have her furniture distrained. But to see her no one could associate her with any belligerent propensities, as she is the type of gentility and charming in her manner. She attended the meetings, heard my address, and I met her at several of the pleasant gatherings to which I was invited. The evening before I left (August 3) she and Mrs. Charles paid me a delightful call. Some time after my return to America I received a clipping with Dr. Veditz's card enclosed. He seems not to have known who the writer was, but I of course instantly recognized Mrs. Montefiore. It appeared in the London Journal *Justice* of August 21, 1909, and is as follows:

PROFESSOR LESTER F. WARD.

One of my most delightful recollections about the gathering at the new Labour College, Oxford, is my meeting in the flesh with Lester Ward, Professor of Sociology at Brown University, Rhode Island. He has been travelling in Europe, and on his way back to the States was induced to spend a couple of days at Oxford, and address some of the young students, who have, under Mr. Dennis Hird, been using his works as text-books in their sociological studies. Ever since some two years ago, our American comrade, Corinne Brown, brought to my notice Professor Lester Ward's "Pure Sociology," and "Dynamic Sociology," I had longed to meet the man whose learning and whose fearless honesty had given us such a wide, profound, and scholarly interpretation of the social facts and conditions that surround us, and of the evolutionary changes that have led up to them. And here he was, a tall, grey-haired man of 70, telling these eager-faced young men how inevitable educationally, socially, and politically was the rise of the people; pointing out that in their struggle they were meeting with precisely the same difficulties, the same prejudices, the same stupidities as did the middle class in their struggle; and that the same inevitable result must crown their efforts as crowned those, centuries ago, of the middle classes. A cultured voice with a slight American accent, a restrained eloquence, a mass of facts, quotations, authorities, a dignity of scientific conviction, thus was the form of the vital message handed down to the spellbound youths who are preparing for their life work in the helping of labour to free itself from the toils of capitalism. Those women and men who were not privileged to hear the verbal message on that memorable occasion, should read "Applied Sociology" which deals with the doctrine of "Intellectual Egalitarianism," showing that brains or intellect are confined to no special class, but only need opportunity. A writer in "Plebs" Magazine considers that among Ward's works, the best text-book for beginners is the "Text-Book of Sociology," price 6s.; and the Professor himself told me he considered his "Pure Sociology" a synthesis of all his former writings and studies. It is of special interest to women, because the fourteenth chapter sets forth in detail his famous theory, based on scientific investigation, of "female superiority" in the slow evolutional process of the development of the sexes. The writer in the "Plebs" Magazine finishes in these words his sketch of this most genia and simple-mannered great man: "When our leaders have time to master his philosophy, there will be room to hope that this earth will become a fit habitation for human beings, for its civilisation will be guided by reason and flooded with beauty."

D. B. M.

The address was published in the *Plebs Magazine*, running through two numbers, and was then reprinted in a neat pamphlet, having for frontispiece one of the photographs of Mr.

Hird and myself taken in Oxford on August 3, and having at the end two fine views of the new Central Labour College. The title given to it by the editors is very appropriate. The reprints were received March 12, 1910.

The Plebs Magazine, Oxford, Vol. I, No. 10, November, 1909, pp. 218–221; No. 11, December, 1909, pp. 241–244; Covered reprint, Oxford, 1910, 8 pages, 2 plates.

EDUCATION
AND
PROGRESS

BY

LESTER F. WARD,

PROFESSOR OF SOCIOLOGY, BROWN UNIVERSITY, R. I., U. S. A.
(Reported by MR. GEORGE GRIFFITHS for, and reprinted from
November and December, 1909, numbers of
"Plebs" Magazine.)

An Address delivered in Oxford, August 2nd, 1909, at the Annual
Meeting of the "Plebs" League.
All information concerning the League can
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LADIES AND GENTLEMEN:—

I PREFER to stand here (on the platform), not because I am so vain as to suppose that there are any members of this audience who care particularly to see me, but because I desire to stand where I can see all the members of this audience. I very well know, from the last two days' experience and observation, that you are all democrats. Of course you know that I am a democrat; you would naturally infer it from the fact that, if I do not represent, I hail from the greatest democracy on this globe. But I want to say more; that my democracy is not merely nominal, not merely political: it is a democracy which is ingrained in every fibre of my nature.

I am very much afraid that you have an exaggerated idea of the message I have to give you to-night. I have heard my remarks spoken of as an address. Certainly that is misleading. The only notes that I have are a few that I jotted down this morning, of which I only canvassed a single one: and I have not had time since, in the intervals of conversation and meeting new friends, to organize or even arrange them.

But even these few notes, were I to attempt to expand them, would occupy much more time than I shall occupy or ought to occupy. The only difficulty is where to begin and what to say. I have been impressed with one thought that is, probably, the most forcible that I have experienced during my short stay in Oxford. I have frequently, or at least occasionally, dropped into or found myself in meetings of bodies of persons who were interested in great reformatory principles and issues, mainly, of course, those bodies which call themselves socialistic, or who represent some great aspect of socialism: and I have always felt a certain thrill when listening to their enthusiastic remarks, and in seeing how intense their interest is in problems of social reform.

But I have always observed this: that the aims that they were seeking were very present, near, close aims; very close home to their own natures and wants and needs. They were demanding, often eloquently, higher wages, shorter hours, and many other great reforms that we know are so much needed in the world for the labouring man; and my sympathies were always with him.

But this occasion differs fundamentally from any that I have ever experienced. You are not here demanding some higher wages or shorter hours; you are not here demanding some immediate material advantages for your physical existence; you are here with equal enthusiasm considering a great problem which you know to be one that involves years, perhaps decades, it may be centuries of investigation, of study, of thought. You are here on a great educational mission; and in that respect this meeting, and this body, differ fundamentally from any which I have ever had the privilege or the pleasure to fall into, or to experience the sensations that their enthusiasm created in me. And I must confess that the very remoteness of your result—which is in harmony with my teachings ever since I commenced to try to influence mankind—has given me a feeling which is far deeper than any that I ever experienced under the other circumstances which I have described.

Now, what are some of the reasons for it? I sometimes undertake to classify reformers. Primarily, we may classify mankind into reformers and non-reformers, because there are some who do not believe in reform for one reason or another; perhaps because they think the world is the best that can be—the optimists, or because they believe the world is incapable of being made any better—the pessimists. But aside from that class, which we cannot now consider, there are the true reformers who do see that the world is not the best it can be, and is to be, and who are working in one direction or another to show how it may be made better. Now, even these reformers may be divided into still further classes; and the first great class, which comprehends the larger number, is the class of which I have spoken, who demand immediate results, who wish to see reforms inaugurated in social conditions around them which they can see to be imperfect and which they might, if they had sufficient power and influence, modify in their own interests and in the interests of man.

I have great respect for them; but my observation has been that very small results have followed from that class of reformatory movement. I have tried with the sociologist to discover the reason for that, and I have endeavored to show what I regard as the reasons. That problem is too large for me to attempt to explain on this occasion, but I may express it in one single phrase and you can supply the balance; and that is that all such reforms are demands to modify social structure. Social structure has that intense stability which is the product of evolution, and there is no possibility of changing the great social structure in a very brief space of time by artificial appliances any more than there would be of changing the character of an animal, or a plant; to change its tissues, its organs, its form. We readily recognize that organic nature cannot thus be suddenly changed; it may be, and has been gradually modified through great periods of evolution. Those periods have been enormous and these changes do take place when we give time enough, but we must go far back through the long vista of the ages.

It is the same with social structures; they have been the product of social development; they are not to be criticized; they have to be examined to see if they have outgrown their usefulness. Social structures have, to a greater or lesser extent, outgrown their usefulness. It is very clear to all philosophers that many such institutions, many such structures, exist; and they are social evils. But whatever they may be, they may all be summed up as cases in which the social structures are no longer perfectly adapted to the environment of society.

Therefore, we come now to the other great class of reformers—namely, those who see that immediate, sudden, artificial changes are impossible in the structure of society. But even that class may be further subdivided; and that involves something of an explanation, and it seems necessary that I should give it as briefly as possible. The great form of improvement that is obvious to that class is that the mental condition of society should be improved. That is obvious to all reformers who see matters in the light that I have explained; but that class may be still further subdivided, as I say; and therefore there do exist to-day two classes of reformers who perceive that the mental condition of mankind should be improved where it is capable of improvement, but they look upon the subject from two different points of view.

One large class, which is most popular to-day, in the scientific world certainly, maintains that what we want in this world is more brains; they maintain that the difficulties under which society labours, the evils under which the lower classes of mankind are suffering, are due to a want of an adequate supply of the qualities of the human brain: and therefore, they are, with Galton as their leader, advocating the process—it is not science, it is art—which they call Eugenics, and it has now become so popular that the London University has a chair and there are one or two chairs of Eugenics in America.

What do they demand? They demand that the mind of man shall be improved by some process, which we may call artificial in exactly the sense that the various breeds of animals and various plants have been improved by human artificial methods. Now, without stopping to develop that, with which you are all probably familiar, I will only say that I do not belong to that school; I belong to that smaller recognized branch of those reformers who see that the human mind needs improving, but who, at the same time, believe that the human brain is adequate to-day—and has always been adequate—for any purposes which it may be called upon to perform.

That is the subject that I have made a study for the last twenty years of my life; and I am so deeply impressed with it that I cannot, perhaps, speak of it with that calmness which it deserves. I will begin by drawing a distinction between the two things, capacity for knowledge, and knowledge itself; or rather, I will define the word intelligence which is not a single idea, but consists of that capacity for knowledge, together with the knowledge added to it; and, therefore, I say that instead of the great demand being that of increasing the human intellect, the native capacity, the hereditary genius of mankind, the great demand of the world to-day should be the increase of human intelligence as thus defined.

If we admit, as I do and as I have endeavored to prove, that human intelligence is adequate for all practical purposes—and always has been—and always will be—then the question is limited down to that simple adjunct, not to the hereditary part which stands upon its own basis and is always incapable of being changed or modified; a limitation which involves all structures even the most delicate. We can leave that to itself, leave it to mankind, to the judgment and intelligence of the world, and restrict our investigations entirely to that second portion of the definition of intelligence—namely, the knowledge necessary to constitute intelligence.

It narrows down then to one point: that the great demand of the world is knowledge. Of course the question is altogether too large for me to attempt to expand it on this occasion, but you are familiar with my books; you know how I attack the problem, and I know some of you are satisfied that I have solved it from that point of view.

If I am right in that, then the great problem is to equalize intelligence; or, to simplify it still further, to put all knowledge in the possession of every human being. I call it the equalizing of intelligence because I maintain that the native capacity of mankind is equal in all classes of society. I have coined a new word in the English language, if I may be allowed to say so, and I have set it afloat and I hope it will be carried to the ends of the earth. I call this the principle of intellectual egalitarianism, the principle that—no matter what class of society you may select from—taking a corresponding number from each—the individuals from all classes of society will be equal in their native capacity for knowledge; that those from any one class will be equal in their capacity for knowledge to an equal number from any other class of society.

It may be said that I have worked that out theoretically; it is not entirely theoretical. I have observed as long as I have lived; but I have never had brought before me any facts that showed with such power and cogency the demonstration of that principle as my experience of the last two days and these last few moments before this audience.

Of course you cannot help seeing the bearing of all this upon the problem of education, but you will all remember—those of you who read my old *Dynamic Sociology* of 1883 know—what I mean by education, because education is not the same thing with me at all as it is with Oxford University. Education with me is exactly that one thing, of imparting to the great mass of mankind, all mankind, an equal amount of the essential knowledge that has been brought into the world.

When you all have that there will be no social classes. Social classes are artificial; they are all made by man; they are all the products of social organization and social phenomena. They are all natural in one sense, in the sense that they have to be, the same as everything else has to be; they have been produced, but they are not natural conditions. Social classes are all artificial. I will go farther, and say that social classes are based entirely—of course you will see the large ellipsis I have to make here—upon inequalities in intelligence, and are the effects of those artificial inequalities of intelligence on different classes of mankind—on different men.

These inequalities tend, necessarily and essentially from their very nature, to produce social stratification; in fact, artificial inequalities have what I call a vertical effect on the stratification of society, raising one man above another in a similar manner to the geological strata of the globe laid down in the sedimentary rocks. That is what happens under existing social conditions and which constitutes the difference between the amount of intelligence possessed by different individuals.

I have said I have come to the conclusion that there is no difference in the native capacity of mankind so far as classes are concerned; that all mankind are equal; that the brain power of the world is the same at every level; even the lowest slaves and serfs that have ever existed on the globe have the same power for exercising their faculties, the same faculties as those who have had them under control, those who have owned them, those who have whipped and flogged them, those who have robbed them of their rights and liberty—there is no difference.

It is sometimes maintained; it has been a stock argument; that if we were to abolish the social classes, it would only be a question of time when the natural inequalities of mankind would bring about a stratification such as existed before. There is nothing more false in the whole domain of social sophism. One single fact will show it; you have here in England—and there have always been here and in all countries where they have had higher and lower classes of society—these inequalities, these natural inequalities, in the higher classes just as much as in the lower. You know perfectly well that a man being a lord of England does not ensure his being a philosopher; and I make bold to say that there is just as much difference in the higher classes, in your nobility, as there is in the lowest strata of your people, the common day labourer who has not so much as the skill of a trade—they all differ.

You ask me, do I deny natural inequalities? Not at all; I admit their influence much farther than those who maintain that they are the cause of social stratification. The great value of human life resides in the fact that the native capacities of mankind differ. And the simple answer is this: that the differences are qualitative and not quantitative. If every man had exactly the same kind of mind as every other man, the world would only have done one thing. But there are millions of men with different kinds of minds, and the consequence is that they have done millions of different things; and that is what I mean by asserting the intellectual equality of mankind, whilst maintaining the essential natural inequalities of mind. There are inequalities in the qualities of minds, and a great number of different kinds of minds; and if we were only supplied with data for exercising those minds we should have in them the bulwark of progress.

I use for the opposite of the natural inequalities the term horizontal, in distinction from the analogy of stratification, as, instead of producing a stratification, the tendency of the natural inequalities is to broaden out the functions and activities of mankind so that every object in the world will be looked into, everything in the world will be heard and seen and known; every thought of the world will be brought out, and the progress of the world will be a broad swath which sweeps over the whole of creation.

If I knew how many persons there were in this room I could tell how many different kinds of minds there were in this room; and, as all minds differ, every one will do something that no one else has done, and so broaden the course of human progress. That is what I mean by natural inequalities; and if there were no social inequalities there would be nothing to interfere with the full exercise of all those capacities of all mankind.

If we look at the question from this point of view, we can readily see what this great natural principle of inequality, or, as I call it, intellectual individuality, or individuality in achievement, is. Achievement has been the result of the immense individuality that has entered into it, due to the vast number and the great difference of human minds.

Now, the only thing that is needed is that all these minds shall be supplied with the means for their exercise. Human minds—admitting again for the sake of argument that these inequalities of the human mind exist—differ entirely in what they hold and not in what they are; and therefore, the great effort is to supply all minds with all knowledge. Of course you know that that is an ideal, but I adhere to that expression: that all mankind should possess all knowledge. I have not time here for the explanation of what I mean by universality, nor can I say now what I mean by universal education; but it is the same thing: a curriculum should actually embrace all the knowledge of the world. ✓

I had intended to go into some other points, but the time is too short. One of these points refers to the proposition that all mankind are equally capable, no matter what position the social conditions may have placed the individual in. I will refer to two, and only two, further points. In the first place, the hereditarian philosophers; those who preach eugenics, and teach heredity, and insist upon an increase in the calibre of mankind: all admit that there have been self-made men all through history, and that there are hundreds of them at the present time. By self-made men they mean men who have risen from the lowest ranks of life to the very highest. What does that mean? It means that down in the very lowest ranks there are just as good brains as in the highest.

The second great proof, for any one who has studied the history of the world, is obtained from history itself. It is from the study of history that we have the knowledge that we now possess of these artificial social classes that have existed during human history. In Greece, at the time of its highest splendour, there were at least ten slaves to one freeman, probably more; in Rome it was about the same. What has become of those slaves? They are now in the other classes of society, they are completely mingled. What became of all the slaves of Europe when slavery and serfdom were abolished? They have all been mingled with the other classes of society. And have they not shown the same ability as their masters did before them? History shows nothing else more clearly than this: that class after class has been rising, one class after another; and the lower classes taking the powers of social organization under their control, taking the reins of human life in their hands, supplanting the other classes.

I trace it back to the system of caste in India, and I have shown that the classes of the eighteenth century were nothing but the holdovers of the old system of caste; that the lords spiritual and the lords temporal were nothing but the representatives of the Brahmins and the Kshatriyas, the spiritual caste and the warrior caste. Those classes have always existed; they existed down to the eighteenth century, and exist in some form to-day in some parts of Europe.

Then take the middle class: that class became, even in India, the business class of society; but in modern Europe that business class took the form of what are called in France the *bourgeoisie*, what you call in England the commons, and they gained the mastery. You know that the middle class, the third estate, came forward and assumed the power in society, and has held that power ever since. The third estate to-day contains and holds the brains of the world.

Many think the present social movement has to do with the French Revolution, that it is a verification of the old falsehood that history repeats itself. Nothing of the kind; the French Revolution had nothing to do with the fourth estate; it was carried on exclusively in the interests of the third estate. To quote Abbé Siéyès: "What is the third estate? Everything. What does it possess? Nothing. What does it desire? To become something." Before the French Revolution the third estate was nothing; it is everything to-day. The rise of the third estate was the French Revolution, and it was a grand work; and it showed that the third estate was capable of supplanting the first and second estates, and it has done so in France. What is the movement to-day? It is nothing but the same great movement. The first and second estates were supplanted by the third, and now the fourth estate is coming on and will ere long supplant the third estate. What do we hear all over the world? Nothing but the subterranean roar of that great mass of mankind, infinitely larger numerically than all the other classes put together; that class is rumbling and seething and working, and coming to consciousness; and when they do come to consciousness they will take the reins of power in their hands, and then will have been abolished the last of all the social classes.

January 8, 1910—Ætat. 68.

552. Spain and Ferrer

History.—After-dinner speech made at the Sunrise Club in New York December 27, 1909. Stenographer's notes revised December 30. On November 24, 1909, I received a letter from Mr. Edwin C. Walker, Secretary of the Sunrise Club, asking me to open the discussion on this subject at the dinner of December 27. I replied on November 27, that I should be in New York on that day and might do so, but I told him that I should leave the declamation and denunciation to the other speakers and confine myself to a discussion of the social conditions that made the execution of Ferrer possible. The same day I wrote letters to Professor Adolfo Posada, Sr. Antonio Ferrer Robert, and Sr. M. H. Cornejo in Spain asking them to write me letters that I could read as part of my address. I also wrote to Professor Ernesto Quesada in Buenos Ayres, and to Sr. Enrique Molina in Chile, asking the same favor of them. My idea was that if I had their letters I could confine myself chiefly to reading them and commenting on them and have something wholly fresh and appropriate to say. I of course told them that I did not expect them to take sides for or against the act, but merely to discuss existing conditions in Spain. But when I left Providence for New York on December 26, no answers to my letters had been received and I had to make such a speech as I could without them. I had really given the matter but little thought, and had only a few heads jotted down to guide me. Miss Bertha W. Howe, reporter for the *Truth Seeker*, was at the same table with me and made an excellent stenographic report of my remarks, which she submitted to me later and I revised it. The subject for discussion at the dinner was announced as *Modern Spain and Francisco Ferrer*, which was abbreviated as above in the report published in the *Truth Seeker*.

On January 3, 1910, I received letters from all three of the persons in Spain to whom I wrote. I had asked them to write in their own language and they did so. They were all excellent and to the point, and carefully excluded everything that could be used against them. If I had had these letters I would have been able to make a most interesting speech. It would not, however, have afforded much comfort to the more ardent class. The letters were conservative, and necessarily dealt to a large degree in generalities, but they were eloquent and elevated in tone. Professor Posada's was the shortest but was warm and enlightened. Sr. Cornejo, whose acquaintance I made in Bern, is the Minister Plenipotentiary of Peru to Spain,* but he is a profound philosopher, and the first volume of his great work: *Sociologia General*, had already appeared when I met him. He gave me a copy of it and I have it, but it cannot be read rapidly. It is a cosmology, and plants sociology on all the sciences. His letter is in keeping with the profound character of his work. The letter of Sr. Robert was much the longest of them all and surveys the whole field. He is, as the reader knows, the person who translated the *Text-Book of Sociology* by Dealey and Ward (see supra, No. 518). He admits that the educational system of Spain is wretched in the extreme, but though living in Barcelona and necessarily familiar with all the conditions that led to Ferrer's execution, he strangely enough has no very high opinion of Ferrer, and while admitting that from the standpoint of civilization at large the act was unjustifiable, he seems to think that from the standpoint of Spanish law and national safety it was no more than the natural result of the state of things and was inevitable. Both the South Americans also responded, but, as I ought to have known, their letters arrived much later and the dinner was long past before they even received my letter. That of Quesada was dated January 10, 1910, and he had then only just received it. He did not consider it necessary to go into detail on the merits of the question (see supra, No. 530). That of Sr. Molina is dated January 7, from his home in Liceo de Talca, Chile, and stated that he received mine on January 3. It gives a fair statement of public opinion in the South American States relative to the execution of Ferrer, which is not very unlike that in the United States. There were numerous indignation meetings everywhere and except by the Catholic clergy the act was universally condemned. Sr. Molina is an active educationalist and has written extensively on the subject. He is laboring with some success for the adoption of secular education in his country. He is fully abreast of the entire thought of the time. I learned to know him in February, 1909, when he sent me his pamphlet entitled: *Un Pensador Norte-Americano. Ensayo sobre la Filosofía Social de Mr. Lester F. Ward*, in which he ably and sympathetically reviews all my books. He has since sent me other of his writings, the last being a critique of the *Pragmatism* of William James, on which he was engaged when he wrote this letter, and which he mentions there, saying that pragmatism, while really a form of skepticism, is pure obscurantism.

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* He has since been elected Senator.

The latter part of this address was reprinted in the *Plebs Magazine* under the title: *A Martyr to Education.*

The Truth Seeker, New York, Vol. XXXVII, No. 2, January 8, 1910, pp. 17–18; The Plebs Magazine, Oxford, Vol. II, No. 1, February, 1910, pp. 20–22.

LADIES and Gentlemen, Members of the Sunrise Club and Guests: I suppose that the purpose of the discussion should naturally be the expression on the part of those who speak, and the voicing by them of the feelings of the rest, of the profound indignation that is felt throughout the civilized world at the commission within the last three months of one of the most barbaric official crimes that was ever perpetrated. I am very sorry to say that, so far as my own humble contribution to the occasion is concerned, I shall not be able to meet your wishes in this regard, nor to say very much of what I imagine is what you have come here to hear. So much has already been said in different parts of the country, not merely in New York but in other cities and also in Europe, that it seems to me the sense which we all feel, that civilization has been assassinated, has already been abundantly expressed. At any rate I shall be obliged to leave that part to the speakers who will follow me, and to devote myself to another aspect of the question, one which seems to me more important even, if possible, than that. What good it can do to express these emotions of ours I leave with you. If it could be made international to the extent that states and nations could actually move in the matter, and by a moral influence at least, if not a physical one, prevent the recurrence of any such barbarities in the future, then the expression of that sentiment might be of some value, but as it seems to me it has expended itself already, and we are all probably very much of a mind on the subject. What I shall say will proceed from another standpoint, which is not a popular one, but rather what I may call the scientific view of all questions relating to human history.

The modern scientific view of history is very different from the ancient one; and when I say ancient, I do not mean so very ancient. One hundred years ago—fifty years ago—it was the practice of historians when they were obliged to record an event of that kind in their histories, to indulge in the expression of their own personal opinions and what they supposed to be the opinions of their readers with regard to the events themselves. The consequence is that the greater part of human action has been more or less characterized one way or the other by able men; and that was the way it was formerly supposed that history should be written. The great crimes of the church in the past in putting to death men whom they were afterwards obliged to honor by statues, were properly characterized as abuses of power in all the great works, and with the proper epithets.

As I said, the spirit of history has changed within the last fifty years, and now we come to regard human events in an entirely different light. And I may say that the change in that view with regard to human history has been practically simultaneous with the origin and the coming to the front all over the world of a great new science which we call sociology—the science of civilization and of human events; and the scientific view has been based upon what we know of science in all other departments of nature. We are coming to look upon mankind as a part of nature and upon human activities as similar to natural phenomena, in all departments. That view has been expressed by many writers. I expressed it thirty years ago in some of my own writings. It is reflected by such phrases as this that "human actions must be looked upon as natural phenomena," or that "we must look upon the actions of mankind as effects of causes." We are beginning to know enough about all branches of science to see that there is no fundamental difference from that point of view among the sciences, and that the science of sociology comes as fully under the great laws of nature as do the other sciences. The consequence is that we look upon human actions and historical events, and all the phenomena belonging to humanity, as the effects of causes and the causes of later effects. They are links in the chain of causation which we can trace back in an almost indefinite regress to the beginnings of things, and which we can trace forward to some slight extent into the great future, and can thereby erect a science which shall promise some possible benefits to mankind.

Now we must look at the case which we have before us from this point of view. Of course it is obvious that if what I have just said is true, there is no more reason for indulging in blame or condemnation of human events whatever they may be, than there would be in the condemnation of earthquakes and volcanos. We do not blame the laws of nature for their bad effects nor praise them for their good effects. What we have come to do in this age of the world is to look upon the phenomena of society in the same light as we look upon the phenomena of physical nature. That, of course, would be very far from excusing any of those actions which we consider prejudicial to the progress of mankind. It is neither praise nor blame; it is simply an attempt to understand why such and such actions, however bad they may have been, occurred in the history of the world.

The question naturally arises, then, What are the causes of this new effect which contemporary history has brought so prominently and tragically to our notice? Is it something that results from the pure caprice of certain individuals, or is it something that has arisen out of the nature of things? If we approach the question without emotion of any kind, we are compelled to make this no exception whatever to the general rule. The most useful thing to do, therefore, if we would obtain any possible benefit from our gatherings all over the world, would be to endeavor to learn what the real causes are that underlie the phenomenon in question.

I make no claim to be able to present these causes to you except in the most general way. The question is, why should such an event have happened in Spain when we imagine that it could not have happened in any other European country? There are two European countries in which such events do sometimes now happen. One is in the extreme northeast and the other in the extreme southwest. The causes seem to be to some extent the opposite, in the sense that in Russia the political power is calling upon the spiritual power to support it in its autocratic or despotic rule; whereas in the Iberian peninsula the spiritual power is calling upon the political power to aid it in keeping back human progress. It seems to me that there is an antithesis here which is very clear.

What is it then that makes such a thing possible in Spain? I have before me one simple fact, expressed in half a line, which is almost sufficient, in and of itself, to explain it. It is this: "Sixty-eight per cent. of the people of Spain cannot read." You have all read that beautiful passage of Victor Hugo in which he describes the philosopher meeting on the street in the time of the great barricades of Paris a man who was breaking the street lamps and doing all the damage he could. The sage expostulated with him in the most eloquent terms, telling him how these modern scientific advances had been brought about, explaining the progress of science and invention. He said, "Have you never read history?" and gave him a long and eloquent lecture. The man gave his answer, and you will all remember what it was, "Je ne sais pas lire!" (I cannot read).

Now, imagine a country in which only every third man you meet can read a newspaper? What kind of a country would that be? It would be one in which just such phenomena as the one we are here to consider to-night would be possible. In a country where the percentage of illiteracy is relatively small, such phenomena are impossible. That, in and of itself, would perhaps explain the whole thing, and would be a sufficient answer to the general historical question of what are the causes that have made it possible for Spain to commit such a barbarity in the twentieth century.

There is one other element, however, which enters strongly into it, quite notably in Spain, perhaps more so in Russia, and that is the enormous inequality in intelligence of the different classes of the people. If all the people were unintelligent, a whole country where no one could read, things of this kind would not occur, or only to a very slight extent. It is the vast contrast between a highly intelligent class in a country and a great mass of leaden ignorance—it is that, perhaps, more than the illiteracy alone. In Russia the students of the universities, the poets and literary men, and the artists are on the side of human progress and opposed to despotism. In Spain it is the same. It is the intelligent classes, the well-informed, who are opposed to those spiritual influences, mainly, the Catholic church in Spain, which hold back civilization. Now it is the dread which this spiritual power has of this intellectual power, small as it is numerically, which rouses passions and makes these things possible. What is not so well known is the fact that there is such an intelligent element in Spain. It is a fact that there is a large element of highly intelligent and cultivated people in Spain; and they are more liberal perhaps than those who are called liberal in England and America, because in those countries of the south of Europe, among the Latin races, there is a fundamental distinction in their mode of thinking from that of more northern people, especially the Teutonic and Anglo-Saxon. The fact that there is no Protestantism, for example, in Italy and Spain, makes some people suppose that there is nothing there but Catholicism, but as soon as anyone travels in either of those countries he finds out to the contrary; still it is true that there is no Protestantism. The mind of the Latin races is so constituted that they never stop at any half-way station. When they get tired of the régime that they live in, they make the whole jump. Some of the most liberal men I ever knew I have found in Italy and among my personal correspondents in Spain. I know the works and personally I know quite a number of eminent writers there.

If you will examine the list of works of Spanish writers of the last twenty-five or thirty years, you will see that there is a large element there who are working with all their might to enlighten their country. Among the more prominent are Joaquin Costa, Pedro Dorando Montero, Giner de los Rios, whose great works on philosophic and religious questions and on sociology as a science have done much to enlighten the intelligent reading people of Spain. I should perhaps mention first of all Professor Adolfo Posada, a great jurist and legal writer, and who has recently written several works on sociology. He has translated one of my own works into Spanish, and others of my works are now in the hands of the translators and will probably soon appear. In Barcelona, where this crime was committed, Dealey and Ward's *Text-book of Sociology* is now being translated into Spanish. But my works are by no means the only ones which are being translated. The advanced literature of the whole world is being placed in the reach of all who can read it. All this serves to arouse an interest in these advanced questions and cause them to be discussed all over the country, and the effect on the other class must be to terrify them. They see what is coming, and they are going to throw obstacles in the way of the progress of this movement. It is this situation which has led to the event which we are here to consider. The religious and ruling classes live on the ignorant masses. The book which your Secretary has just named, *The Martyrdom of Ferrer* by Joseph McCabe, and another book published in France by the French committee organized to inquire into the causes of such phenomena in Spain and other countries, set forth in the strongest possible language the means and methods by which the present powers of those countries operate to check civilization and progress.

Ferrer started out with the idea that by forming coalitions and associations with liberal-minded men and making revolts and revolutions here and there, something might be accomplished in advancing the state of his country, but every attempt that he made in his early life absolutely failed, for the reason I have described, ignorance, with its superstition, its subjugation to spiritual and priestly influence, which made his efforts wholly nugatory. He started out at twenty with those ideas; at fifty he had completely outgrown them, and came to the conclusion that that was not the way to reform the world, that it must be far more fundamental, that it was a question of intelligence. He therefore plunged into this great work of education for which he will always be famous. At the time the riots occurred of which he was perfectly innocent, in fact which he would have opposed, he was condemned at fifty for what he said at twenty and had entirely abandoned.

The special interest in this particular event lies in the fact that

Ferrer was a martyr to the principle of education. There have been martyrs to religion, and to science, but never before was there a martyr to education. Ferrer saw that education was the great weapon. Now, it takes considerable intelligence and close insight into human nature, great knowledge of history and power to analyze events, in order to dislodge erroneous ideas, even in our own society, to rise to the conception that education is really a panacea for our social evils. Why is it so difficult to see that education will do this? It is because we are laboring under an erroneous world view, as I call it. We are laboring under the idea that there are great natural differences among men, that the lower classes are naturally inferior to the upper and middle classes. Now, in order to see that education can accomplish the reforms that the world is attempting to-day we are obliged to penetrate into that world view and to see its error. We are obliged, in other words, to recognize the natural equality of all men, for which I have coined a word, namely, egalitarianism. It means simply that no class has or ever did have a monopoly of brains; that there are just as good brains among the "mudsills" and the laboring men with their picks and shovels as there are in the higher walks of life. What then is the difference, and why is it that we have intelligent and unintelligent classes? Because the unintelligent classes are uninformed, uneducated. Educate them and they are our equals. Ferrer saw that. I do not know whether he ever said it; I have said it. Already there had been intimations of it. There have been philosophic glimpses of the truth. Bacon said "Knowledge is power," and we have a proverb that "Intelligence will rule," and both are perfectly true. But that does not do away with the error that prevails in the public mind. It does not teach that the power that knowledge confers it will give to anybody to whom the knowledge is supplied; that intelligence is possible to all mankind; that there is no class in society that is not capable of becoming intelligent if it can only be instructed. All classes are equal when all are equally equipped. But we are living in quite a different world view, viz., the one that I have characterized as the oligocentric world view, the view that the few are all there are of any consequence in society, that everything should be done for the few, that the great mass are of no importance to mankind but are simply used and exploited by the few, the idea which has come from antiquity that "humanity was made for the few." (Lucanus). We are living still under that error and we must get rid of it. Ferrer worked himself out of it.

The fact is the few have been ruling the world all these ages. The tail has been wagging the dog. All we want is that all the elements of society shall be brought into action. Ten per cent. of the world to-day is ruling ninety per cent. We hear about a "submerged tenth," but there is a privileged tenth that is no better than the submerged tenth, and the intermediate eighty per cent. is just as good as the privileged tenth if they can only be given the proper equipment for work in the world. History has shown it. So we may translate the expression which I have just used that all are equal when they are equally equipped, into this slightly changed form that all are equal when they are equally informed, and when all are equally informed exploitation and barbarisms of this kind become impossible.

What does the present unrest in the whole civilized world mean? It means this same truth, that knowledge is percolating down into the great leaden mass. What we hear is nothing but the great Demos turning in his procrustean bed, beginning to roar, with a voice that is low and hoarse it is true, but which is so powerful it makes the whole world tremble. All countries are awaking, and Spain will awake whenever she is vouchsafed the Modern School.

[In his closing remarks, Professor Ward spoke in part as follows:]

Mr. Hall's remark that there are no bad people in the world reminds me of what Buckle says in his *History of Civilization*, to the effect that even the Spanish Inquisition was the work of society and not of the particular inquisitors whose names appear upon the annals of history, the populace in all cases demanding the punishments which were inflicted upon the heretics.

Speakers have referred to the quality of education as distinguished from its quantity. What is education? My idea of it is totally different from that which prevails in our highest institutions to-day. From what I know of Ferrer I mean very nearly the same thing by education that he meant by it. I define intelligence as "intellect plus knowledge," and I embody in the word "knowledge" the whole of education. The education which does not give knowledge is not education. The whole educational problem in this country and all other countries is in a state of utter chaos. Education means "the diffusion of knowledge among men," to quote the celebrated phrase of James Smithson. It is knowledge that is going to regenerate the world. There is only one kind of knowledge, a knowledge of this world, of the universe, of our environment, of our race—its origin, development, history, and evolution. And that is exactly what Ferrer tried to impress upon the students of the Modern School.

March 23, 1910—Ætat. 68.

553. Sociology and the State

History.—Written December 1–19, 1909. Read December 31, 1909. On May 4, 1909, I received from an unknown source a copy of the New York *Evening Post* of May 1, containing a marked article under Literary News and Reviews entitled *The Pretensions of Sociology*, and signed Henry Jones Ford, Princeton, N. J. It was a general broadside attack on sociology, differing in no important respect from what we had long been accustomed to. He quotes a number of American sociologists, including me, selecting isolated passages and interpreting them for himself to the disadvantage of the writers, and, as I say in this article, indulging in low appeals to popular prejudice. The following is an example.

> "Robust sociologists contend that we should all be as free to find our affinities as cats and dogs. Suggestions of trial marriage are made simply as a temporary palliative of an enslaving institution. The trouble with divorce laws is not that they are loose, but that there should be any laws at all. Human beings should be free to mate as they please, like other animals enjoying their natural freedom."

There are also appeals to religious and political prejudices. I state in the paper the unfair way in which he gives my theory of the state. The article was simply written *ad captandum vulgus*, and I paid no further attention to it than to send him my address on the *Establishment of Sociology* (see supra, No. 530).

I was abroad during the summer, and on my return I was surprised to find not only that Ford's article had been published in full in the July number of the *American Journal of Sociology*, but that Dr. Small had headed that number with a *Vindication of Sociology*, which is an attempt to answer it. Professor Charles

A. Ellwood was also allowed to publish *A Reply* in the same number, which would be more damaging to sociology than anything that Ford had said, characterizing Herbert Spencer and myself as "quacks and visionaries," and "at bottom anti-Darwinian"! Fortunately the columns of that journal were opened to Professor Ford for a second article in the September number, entitled: *The Claims of Sociology examined*, in which he set forth his wonderful theory of the state. *Si tacuisses philosophus mansisses!* At the end of a letter from Dr. Small dated October 2, 1909, he says:

> "Perhaps you have not noticed that during your absence Sociology has been destroyed, root and branch, by Professor Ford of Princeton. For the obsequies, see *A. J. S.* for July and September."

In my answer to this letter dated October 5, I said that if the matter was to be further discussed I might have something to say about Ford's theory of the state. On October 23, I received another letter from him enclosing a letter from Ellwood to Ford and the latter's reply to it, upon which he asked my opinion. It was evident that Ford was weakening. I have now forgotten what I wrote; but I returned the letters with a brief comment. Dr. Small had said nothing to me about taking part in the discussion, but on November 27, I received a letter from Professor Giddings saying that Dr. Small had told him that I would be willing to read a paper before the forthcoming meeting of the American Sociological Society, in which I was prepared "to say something suggested by Henry Jones Ford's outbreak." I replied the next day expressing a willingness to do so, and he then asked me for the title to enter on the program, and I gave it as it still stands. I spent considerable time hunting up the writers who had condemned sociology, and also those who maintain that there are no true laws at work in society, and did not get ready to write the paper till December 1. I made a rough draft which was finished on December 3, and which I have preserved along with the data upon which my statements are based. I did no more work on it until the 17th, when I took it up and reduced it to final form to read before the Society. This occurred on December 31, in New York, and I handed the paper to Dr. A. A. Tenney, who had been made Secretary of the Society.

It took its regular course, and I received proof on March 4, 1910. It appeared on March 25, and I received reprints on April 12.

This paper was reprinted nearly entire in the *Plebs Magazine*, running through two numbers.

The American Journal of Sociology, Chicago, Vol. XV, No. 5, March, 1910, pp. 672–680; Publications of the American Sociological Society, Vol. IV, Chicago, 1910, pp. 192–200; The Plebs Magazine, Oxford, Vol. II, No. 5, June, 1910, pp. 108–111; No. 6, July, 1910, pp. 129–131.

June 16, 1910—Ætat. 68.

554. The Historical View of Woman

History.—Written May 15–19, 1910. On May 14, 1910, I received a letter from Mr. Hamilton Holt, enclosing proof of an article by Mrs. Annie G. Porritt, entitled: *Woman as a Metonymy*, and asking me to write a reply to it to follow it in the *Independent*. He said he was asking Professor W. I. Thomas to do the same, but he afterwards learned that he had gone to Europe. I read the article of Mrs. Porritt and decided to comply with Mr. Holt's request, wrote him to that effect the next day, and also wrote the rough draft of the article. The final draft I wrote on May 19, and sent it on that day. Proof came on June 4, and the number containing it was out on the 15th, a day ahead of date. This article was reprinted entire in the *Daily People* for June 26, 1910.

The Independent, New York, Vol. LXVIII, No. 3211, June 16, 1910, pp. 1326–1328; The Daily People, New York, Vol. X, No. 361, June 26, 1910, p. 3.

THAT men do not fully understand women is only a part of the wider truth that mankind do not understand themselves. Neither sex understands either itself or the other sex. Though man is doubtless the proper study of mankind, all the study of man since Pope's day has taught mankind very little about man. This article of Mrs. Porritt is an excellent illustration of this.

The only way to understand the sexes is to learn their history and the history of sex itself. This carries the student back not merely to the remotest ages of the human race, but to the beginnings of life itself.* Such preparation is not vouchsafed to the present generation, which, in its exclusive study of "the humanities," wholly neglects the study of humanity.

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* See "The Past and Future of the Sexes" in this journal for March, 1906 (Vol. LX, No. 2988, pp. 541–545); "Pure Sociology," New York, 1903 (2d Ed., 1907), Chapter XIV.

The writer of this article, though a keen observer of human nature, seems to be as innocent of the historical perspective as the rest of the world.

There are many paradoxes besides the one that Mrs. Porritt points out, viz., that women are less emotional than men in the sense that they are "of a much cooler and less excitable nature." It does seem strange to any one capable of seeing things "objectively" that woman should be proverbially regarded as "fickle and changeable" (*varium et mutabile*), when she is clearly the conservative element in society, and the balance-wheel, as it were, of the race. All this shows that no amount of evidence can change a current opinion. The current philosophy is not based on evidence. It is based on historical events. It is a social structure that has been developed in much the same way that organic structures have been developed, and reason has no more to do with the one than with the other. The opinions that men entertain relative to women are no more the result of their rational faculty than is the color of their eyes or hair, and they can no more change those opinions by taking thought than an Ethiopian can change his skin or a leopard his spots.

The "male dominance" of which Mrs. Porritt speaks comes down to us from the remote age of androcracy which supervened upon the metronymic period in prehistoric times. It is capable of a historic explanation, as are all the other stages in the history of the sexes. Its present form is very mild compared with earlier forms, and the progress that has taken place in the mitigation of woman's condition is the surprising fact, rather than the existing inequalities. But this, of course, should not deter any one from striving for the ultimate removal of those inequalities. That this progress is more rapid now than ever before there is no doubt, and it will probably go on, like most other forms of progress, at an accelerated rate. The direction which it should take at present is that of securing woman's economic independence. This first of all problems Mrs. Porritt does not so much as allude to.

As regards the relative emotionalism of the two sexes, much might be said. While it does seem to be true that in matters of love woman's attitude toward man is more calm and controlled than man's attitude toward woman, it cannot be safely inferred from this that woman is less emotional than man. Her rôle has always been that of selection, and her attitude is naturally defensive. Her long schooling in this line has made her the cautious sex, while man is essentially reckless. This must not be mistaken for greater rational deliberation in women. It is the manner in which they have evolved, and is no more rational than man's aggressiveness. Both may be called instinctive.

When it comes to the really "objective" or "detached" study of things in general, there is no comparison between the sexes. The male sex is almost the only one capable of this. Women, as has been often pointed out, take little interest in abstract questions, questions which do not concern their personal well-being or the safety of their charge. This can also be explained historically, and is not a necessary condition, but can be and is being altered by the scientific training of women.

Most writers of both sexes regard woman as the more highly developed sex from the affective side. Comte called her the "*sexe aimant.*" The "lying lines," quoted by Mrs. Porritt, and, as she says, "written of course by a man" (Byron), are exactly paralleled by those of a woman, Madame de Staël, perhaps written earlier, where she says that "love is the history of a woman's life; it is an episode in man's."

Mrs. Porritt is of course obliged to admit that most human achievement has been the work of men, and her explanation is characteristic of the unhistorical method of treating such questions, viz., as "due, not so much to any superiority of intellect, as to greater driving force which men have available for their work on account of their more powerful emotions." That certainly is a strained explanation. But is there any truth in it at all? It may be admitted that man's sexual emotions, in common with those of the male sex of all species, are more acute, more imperative, and more powerful than those of woman, but can the same be said of his other emotions? Probably not, and only a very small part of achievement is connected with the sexual emotion. But achievement is not the result of emotion. A certain amount of interest and even of enthusiasm is essential, but, given this, the real work is that of intelligence, skill, training, and equipment for it. Even genius is largely the product of opportunity.

John Stuart Mill, though the proper data for such a study did not exist in his day, saw the true explanation of woman's inferiority in the great lines of human effort. He justly said that women had had no chance to show what they could do, that they were not professionally trained for anything, as men are, that they were therefore virtually all amateurs, and that the only just comparison would be of all women with amateur men. Such a comparison would certainly be creditable to women, for it may be said of amateur men, much more truly than of women as a whole, that they have accomplished nothing in the work of the world.

But when we bring the searchlight of history to bear on this question it becomes one of the simplest in the whole range of evolutionary problems. The race has lived under the androcentric philosophy since long before the dawn of recorded events. Vast prehistoric stretches of the androcratic régime prepared the way for the patriarchal system, and woman was loaded with the whole weight of that system before history began. The earliest literature is permeated with it, from the Code of Manu to the Eumenides of Æschylus. It created the *patria potestas* and the Salic Law. It cropped out through medieval history in codes and pandects, in papal bulls and Witch Hammers. Woman, as Mrs. Porritt says, became "a lure of the evil one, an emissary of the devil, something to be shunned and abhorred, to be forever abjured by the ascetic who would save his soul by self-denial." Under such an incubus what could woman do? There was nothing for her to live for. None of the great vistas of fame were in sight of her. No avenues of renown or even of happiness were open to her. The codes of law discriminated against her at every point. She could propagate the species or she could go into a convent, but beyond this her life was barred. The utter absurdity of expecting any great achievement from a sex thus circumscribed would seem to be apparent to any rational being. Still worse is the tendency to blame the sex for it, or to impute it to any natural inferiority. But as remarked at the outset, men's judgments on such questions are not based on evidence or logic or any form of reason. They are the products of the system of thought that has been evolved during the history of mankind. It is also unfortunately true that the kind of education that is doled out, even still to-day, and in the highest institutions we have, fails to enlighten even the few who could and would reason, on the simple historical facts which so perfectly account for the present relations of the sexes.

555. [Solidarité Sociale.] Paroles

History.—Remarks made July 20, 1909. Written out August 25, 1909. I was very unexpectedly called upon at the opening meeting to speak to the Congress on the progress of sociology in America since the last Congress, I being the only representative present from this country. I was asked to speak in English, but I preferred to explain my unprepared condition in French, and the first paragraph was spoken in French, the rest in English. Dr. Worms asked me to give him my remarks in writing, but I did not have time to write them out till I got back to America. On August 22 I wrote out that speech in English. I remembered every word, and I do not think a dozen words are different from the ones used. On the 25th I put it into the best French I could muster. There was no one at Brown to submit it to, but scarcely any changes were made. As there will be no English version, I will introduce it here from the retained copy made before sending it to Paris. It is as follows:

There is nothing more inspiring than to contemplate the formation of so many organizations all over the world devoted to the scientific study of social phenomena. The movement in this direction is world-wide and spontaneous. Your Secretary has enumerated most of the societies of this class. The International Institute of Sociology was the forerunner of them all, and doubtless greatly facilitated the formation of others. But it was itself in large part a product of the age. The world was ripe for it. It is always a question in such cases as to how much is cause and how much is effect. In one sense all these organizations may be looked upon as products of the *Zeitgeist*.

Since the last Congress of this Institute there has been founded *The American Sociological Society*. You all know its history, but as I have been closely connected with it, it seems proper that I should mention it. Three meetings have been held and a large number and variety of subjects have been somewhat thoroughly discussed. Although these subjects have not been confined to theoretical sociology, and practical problems have been introduced, I think

I can safely say that the proceedings have been held strictly within the limits of scientific investigation, and that all wild schemes and declamatory methods have been rigidly excluded. The papers have all been published together with the discussion thereon, and to them I refer you for the proof of this statement.

Viewed in the large all sociological problems may be arranged under two general heads, and these heads may be designated *social struggle* on the one hand, and *social solidarity* on the other. These are the two great centrifugal and centripetal movements in the social world. Both these movements have their exponents in all countries. The first has naturally attracted much the larger share of attention. The social struggles of history may be distinguished as the religious struggle, the political struggle, and the economic struggle. There is also what may be called the social struggle in the narrower sense, which I distinguish from the economic struggle. The religious struggle is happily now almost a thing of the past in all civilized countries, and let us hope that the past two weeks in Geneva have buried the last vestige of it. The political struggle is nearing its end, and in most countries political liberty has actually been attained. The economic struggle still rages, as does also that deeper struggle for social freedom. It is these that form the chief topic of present-day study on the part of scientific sociologists. The great prominence of this aspect is apt to exclude from view the second more quiet but equally important movement, the tendency toward solidarity.

Both of these points of view have their special students in America as in Europe. Of course the struggles attract by far the greater number, and these also are much louder in their methods of dealing with social problems. They are not always scientific, shading off into pure advocacy of supposed remedies for social evils. From this to heated reformers and violent agitators is only a series of steps, but these, as already remarked, are carefully excluded from scientific bodies. We have, however, at least one prominent American sociologist who has founded his system upon what may be regarded as the fundamental principle of solidarity. I refer to Professor Giddings and his doctrine of "consciousness of kind." He sees the whole science of sociology in the light of that principle. Now there is no doubt of the existence and the great influence of social attraction, but neither is there any doubt of the existence and power of social repulsion as manifested in the different forms of social struggle. It is unfortunate that sociologists should thus be divided into these two opposite camps, those in the one camp being scarcely aware of the existence of those in the other.

I have characterized the social struggle as centrifugal and social solidarity as centripetal. Either alone is productive of evil consequences. Struggle is essentially destructive of the social order, while communism removes individual initiative. The one leads to disorder, the other to degeneracy. What is not seen—the truth that has no expounders—is that the wholesome, constructive movement consists in the properly ordered combination and interaction of both these principles. This is *social synergy*, which is a form of cosmic synergy, the universal constructive principle of nature.

It is the special merit of the International Institute of Sociology that it has perceived the great dualism of which I have spoken, and has devoted one entire

Portrait of the Author

At the age of 69

Congress to the discussion of Social Struggles, and is now to devote another to Social Solidarity. By thus grappling with both classes of problems the relations which they sustain to each other can be clearly seen, without which it is impossible, but with which it is easy to study this great rhythm and to comprehend the equilibration that is going on among all these great social forces.

Annales de l'Institut International de Sociologie, Tome XII, Paris, 1910, pp. 85–89.

December 1910—Ætat. 69.

556. [War and Progress]

History.—Remark made from the chair July 21, 1909. Written out August 26, 1909. I had been asked to preside over the afternoon session of July 21. Novicow, in his paper on *L'Essence de l'Association Humaine*, got back on his hobby about war and peace and quoted and attacked me in the same way he had done before (see supra, Nos. 533, 535). It was the same old story and nothing new. It aroused an animated discussion, and Worms, Roberty, Grimanelli, and others defended me. I know I mentioned Roberty, but for some reason his remarks do not appear in the *Annales*. This is probably why his name is omitted from my remark.

Ibid., Tome XIII, Paris, 1911, p. 49.

December, 1910—Ætat. 69.

557. [The Hierarchy of the Sciences]

History.—Remarks on the paper of Dr. Wilhelm Ostwald on *Energétique et Solidarité*, read July 22, 1909. Written out August 26, 1909. I spoke mostly in English, and translated it into French after my return to America. As in the case of my remarks at the opening of the Congress (see supra, No. 555), I will put the English into this sketch.

I was very much interested in Dr. Ostwald's diagram and there is much I would like to say in its praise, but that would be a waste of precious time. I will therefore devote five minutes to making a few criticisms.

The series of sciences that it represents was specially stated to be substantially that of Auguste Comte. Now it should be pointed out that Comte did not introduce logic, which here stands at the base, but on the contrary expressly rejected it as not a science at all. As regards mathematics, here placed next, it is well known that Comte did not regard it as a science similar to and coordinate with the other great sciences dealing with the phenomena of nature. With him mathematics was simply the norm of all the sciences, the test or criterion of the degree of exactness or "positivity" of each of the sciences, by which its position in the series is determined.

I am not aware that Comte ever spoke of a "pyramid." He called the system a "hierarchy." The term was not happily chosen, as the sciences of his series have no rank, implying superiority and subordination, as in a true hierarchy. But it is too late to get rid of the term. It has become universal and common to all languages in the sense in which he used it, which everybody now understands, and no one ever thinks of its literal meaning.

But Dr. Ostwald's figure is open to a more serious criticism than these merely verbal ones. A pyramid does not at all represent the process of development of the sciences. The Comtean hierarchy presents the order in which the phenomena of nature have unfolded in the history of the universe. That process is one of *evolution*, and Comte constantly used that term long before Herbert Spencer had outlined his system. Now evolution is a process of upward expansion. It proceeds from the general and simple to the special and complex. Its special characteristic is differentiation. It is a ramification. Its form is arborescent, broadening out from below upward, from a simple tap-root in the lower sciences to a great complex summit of myriad interlacing branches and branchlets in the biological and social sciences. This form, although it has no inherent resemblance to a pyramid, still, if any one chooses so to designate it, is that of an inverted pyramid. Dr. Ostwald's figure is wrong side up, and must be reversed if it is to represent the true evolution of cosmical energy.

Ibid., pp. 147–149.

July 28, 1911—Ætat. 70.

558. [Note on a Proposed trip to Christiania, Hamburg and Rome]

History.—Written July 21, 1911. On May 17, I had received a note from President Faunce asking me if I would go to Christiania as delegate from Brown University to the centenary jubilee. The congress of the International Institute of Sociology was to meet in Rome on October 14, and I was preparing my papers on social progress, which I expected to send on to be read, as I did not suppose I could go. I consulted with Professor Dealey, and he offered to conduct all my classes till my return. He saw President Faunce on the subject and he was willing to let us settle the matter between us. He sent me the Latin invitation to send a delegate and I finally decided to go. I received my regular credentials on July 10, while I was in New York giving the course of lectures at Columbia University. On July 20, I received an invitation to attend the Hamburg Congress of the Monistenbund to begin on September 8. I saw that by leaving Christiania as soon as the jubilee was over I could take that in on my way to Rome. It was then that I completed the plan of the trip and thought I would announce it in *Science*. It was just what everybody does. If the parties to it do not announce it there will be no announcement, and I knew that many of my friends would be interested in the matter. The program was carried out except as to the Rome Congress, which was postponed on account of the cholera in Italy, and I returned two weeks earlier than I expected.

Science, New York, Vol. XXXIV, No. 865, July 28, 1911, p. 108.

January 12, 1912—Ætat. 70.

559. Review of "The Changing Chinese, Oriental and Western Cultures in China." By Edward Alsworth Ross, Ph.D., LL.D., Professor of Sociology in the University of Wisconsin. New York, The Century Co. 1911. 8°. Pp. xvi + 356

History.—Written December 13–20, 1911. I received the book from the publishers, whom Dr. Ross had requested to send it to me, on October 31. I had already read several of the articles in the magazines, and did not at once commence its systematic reading. But I immediately sent him a note in these words, as nearly as I can recollect:

> "Changing, yes, and faster than you thought. You have struck a tida wave. May you ride on its crest to immortal fame as the Prophet of the Orient."

Every one who spoke or wrote me of it mentioned its opportuneness. I soon received a long letter from him, dated the same day (November 1) as mine to him, in which he said that they were all saying the same thing to him, and that he told them that he instigated the Chinese rebellion as an advertising dodge. I read the book systematically and carefully, not omitting the chapters I had read in the magazines, from November 30, to December 6. I found that parts not previously read were by far the most interesting. Before writing the review I went through and took off a long string of points marginally noted by me while reading. In the review I could not canvass a fourth of them. I had asked Dr. Cattell if he would like a review of this book for *Science*, and he had stated that he would. I mailed it to him on the day it was finished (December 20). In the letter of transmittal I said nothing about going away during convocation week, presuming I would be back before the proof would arrive. I returned on January 2, 1912, and found the proof in my mail post-marked December 26. If I had been there to read it I presume the review would have appeared a week earlier, that is in the issue of January 5, or the first number of Vol. XXXV. While in Washington during that week I met Dr. Ross and we had some good old-fashioned talks, discussing the book especially, but I did not tell him I had reviewed it, as I wished it to be a surprise. I mailed him a copy of *Science* containing it on January 13. I asked for fifty copies and distributed half of them at once.

Science, N. S., New York, Vol. XXXV, No. 889, January 12, 1912, pp. 64–68.

February 17, 1912—Ætat. 70.

560. The Struggle of To-day

History.—After-dinner speech made at the Sunrise Club in New York, January 29, 1912. On December 4, 1911, I received an invitation from Mr. Edwin C. Walker, Secretary of the Sunrise Club, to speak on the above-named date, which, being the 175th anniversary of the birth of Thomas Paine, was to be the occasion of certain doings by the Thomas Paine Association in New York. The meeting of the Association was to precede the dinner in the same building, Café Boulevard, Tenth Street and Second Avenue, and the speech-making was to follow the dinner. I was not even a member of the Association, although I had been urged to be its president when it was founded two years before. Mr. Thaddeus B. Wakeman was made its president. As he was very prominent at the International Congress of Monists which met in Hamburg in 1911, accompanied the large delegation that went to Jena after the Congress, and was the bearer to Professor Haeckel of a memorial from several liberal organizations in America, including the Thomas Paine Association, delivered a set address to Haeckel in presenting the memorial, and was in many ways associated with the movement that Haeckel represents, it was proposed to connect the two movements on this occasion and make it a kind of Paine–Haeckel affair.

As I had also attended the Hamburg Congress and went to Jena with the delegation, I wanted to say something about Haeckel if I spoke at all, and I intimated as much to Mr. Walker. But on January 15, 1912, I received from him the program of the speeches. There were six speakers, of whom I was the last. I had told him that I must return to Providence on the midnight train, as I had two examinations to hold the following day. As the speeches might occupy forty minutes it was evident I should have to go before I was reached. Moreover, the subject assigned for my remarks was: *Sociology—Its Beginnings as a Science, Its Growth, What it Means for the Race.* I immediately wrote to Mr. Walker that I should not come, giving both reasons, though either one would have been sufficient. But he replied that I might speak on anything I pleased, that he would see that I was called by nine o'clock, and that his reason for putting me last was to hold the audience! He begged so hard that I finally concluded to go. He kept all his promises, and I had no ground for complaint. Indeed he announced that I should speak on Haeckel instead of sociology. The menu was headed by pictures of Paine and Haeckel side by side. Mr. Wakeman led off in the speaking and confined himself to Paine. The affair was a great success but I was obliged to leave before the speaking was over. I had made no preparation whatever, and depended wholly upon the occasion. Miss Bertha W. Howe, who reported my other Sunrise Club Dinner speech (see supra, No. 552), was present and took down this one admirably, leaving blank spaces for big words she could not catch. She sent it to me in neat type-written form and I corrected it and sent it direct to the editor of the *Truth Seeker*. I enclosed one of the prints I had made in 1903, with my signature underneath, but the picture that accompanies my remarks as printed in the *Truth Seeker* is a different one. I did not recognize it, and looked over all the pictures I had or that had been published among all the sketches that had ever appeared without finding it. But Mrs. Cape, when she saw it, recognized it as an enlargement of a picture which occurs in a group of the persons who waited on Professor Haeckel on the morning of September 13, 1911, mentioned by me in my remarks. This group had been published in the *Truth Seeker* for November 18, 1911 (Vol. XXXVIII, No. 46, p. 729). They had isolated my picture in that group, enlarged it, and introduced it here, placing beneath it the signature I had written on the print that I sent with the proof.

My remark about being disqualified for speaking on the subject of sociology needs explanation. Mr. Wakeman, in speaking of Paine characterized him as one of the founders of sociology, and went so far as to say that the sociologists were all going wrong in not starting from his teachings. The idea was sufficiently absurd, and I disposed of it in a word.

The Truth Seeker, New York, Vol. XXXIX, No. 7, February 17, 1912, pp. 98–99.

REMARKS OF PROF. LESTER F. WARD AT THE PAINE ANNIVERSARY DINNER IN NEW YORK, JAN. 29.

SINCE receiving the invitation to attend this dinner and make a few remarks my mind has been dwelling on the two great men whose names appear on the program, and their work; for one reason, perhaps because I met for a few moments not long ago the one of them who is still living, Professor Haeckel. I was interested in Haeckel particularly, presuming that the president of your Paine Association, Mr. Wakeman, would do full justice to Paine. In giving a somewhat qualified acceptance of the invitation, I had thought of saying a little something about Haeckel and not so much about Paine or sociology. I am a professor of sociology, technically. It is the subject that I spend all my time upon, and upon which I give nine lectures a week, and it did not seem anything very remarkable or interesting to me to come down here to New York and talk half an hour on that subject. I have learned since the speaking began to-night, that I am disqualified to speak on that subject, because I certainly did not begin with Thomas Paine. In fact, I am inexcusably ignorant of Paine. I have several times tried to read some of his books. The only one that I made any headway with at all was his "Age of Reason." That was forty years ago, and I did not get far with it because I found, even at that date, that I was so far ahead of him.

To have discussed the subject of sociology would have been possible, perhaps, if we could have had Professor Loeb's speech on biology. It would have been a natural transition from biology to sociology. I make great use of biology in teaching sociology. I should have been delighted to hear what Professor Loeb would have had to say on the subject, and I might have supplemented it from the standpoint of sociology. But as I have now permission to speak on Haeckel, perhaps I had better devote the few minutes I have to saying some things about him.

It is well known to many here, besides those of you who were present, that on September 12 and 13 last, Haeckel was visited by a delegation from the great International Monistic Congress that was held in Hamburg, which he could not attend on account of injuries which he had received from a fall in the previous April. The delegation consisted of three hundred members from various countries who went from Hamburg to Jena on purpose to see him, and who gave him a reception there, or rather an ovation, if you may call it so, such as any man, I think, might be proud of; certainly I know that Haeckel was proud of it. The first evening there was a torch-light procession more than a mile long which marched past his villa, stopped in front of it, and heard him speak from the balcony. The next day a small deputation called on him in his home. Professor Wakeman was the leading speaker of that particular occasion, and read before him a memorial that he bore from various associations, including this one, which he represented.

I had not seen Professor Haeckel since 1894, when by a pure accident I met him on top of the Uitliberg above Zurich on the occasion of the meeting of the International Geological Congress of that year. We had had a long correspondence, but had not expected to meet there. However, I was delighted to see him, and I believe he was glad to see me. It was one of the most enjoyable events of my life. We sat out on the verge of the mountain just before sunset and saw the sun go down over the peaks of the Alps, while we talked on the subjects in which we had a common interest.

On the recent occasion to which I have referred, when I was for some reason made the first to address him, the first thing he said to me was, "Do you remember the time when we met on the summit of the Uitliberg and saw the sun set over the Alps?" and I told him I should never forget it, but I didn't know whether he would remember it. He said, "I shall always remember it."

Since returning to America I have received a very interesting letter from him, which I hold in my hand, but it is too long to read to you. He says among other things, however, that he is seventy-eight years of age and we cannot expect much more from his pen. "But at least," he says, "I have had the satisfaction of seeing three hundred editions of my "Welträthsel" appear, of one thousand copies each." Three hundred thousand copies circulating in German-speaking countries! You know that that work, and also his "Wonders of Life," have been translated into English and are circulating here. He sent me the originals, and also the English, or, rather, American editions. In his letter Professor Haeckel adds: "Besides twenty-one different translations, the last being into Hindustanee." So you see that Haeckel is a man whose influence is enormous, and deservedly so.

In his letter he encloses a picture, the one familiar to most of you, in which he stands beside the skeleton of a gibbon in his museum. He also sends me a very fine picture of the Phyletic Museum, which he built out of his private fortune and presented to the University of Jena on July 30, 1908. On that occasion he made one of his celebrated addresses, which he sent me in a separate wrapper. I have since ordered and received the great illustrated folio monograph, "Unsere Ahnenreihe" (*Progonotaxis hominis*), to which the address was merely an introduction.

Of course we have always exchanged publications, but I do not count my effusions as any equivalent for his works. I might, perhaps, go back a moment to the time when his "Anthropogenie," or "Evolution of Man," as the English editions call it, appeared in 1874, of which I wrote an extended review running through three numbers of an American magazine. Of course it included also much relative to his previous works, his "Generelle Morphologie," 1866, "Schoepfungsgeschichte," 1868. That was in 1877. Think of the date of it! And two years later I had those articles stitched together and one thousand copies published in a neat pamphlet, "Haeckel's Genesis of Man." I did not suppose that the public at large would care much for it, but my friend, Professor Wakeman here, long before I had any personal acquaintance with him, in some way got hold of it, read it, and went all through this country lecturing and announcing it to his audiences, and it was not a year before I was obliged to recall a small remnant in order to have any copies of it to give to my friends.

I might say one or two words about Haeckel's views on other subjects besides biology. Of course, he is the great biologist of to-day. He is the Darwin of Germany, and he far out-Darwins Darwin in many respects. He has the courage of his convictions, and not only that, but he has the spirit of propagandism, as you may call it. He has the feeling that "What I know is valuable, and the world ought to know it, and I am going to tell it to them." He is a philosopher as well as an investigator, and he has in his works, as you know, uttered so many unpopular truths that he has incurred the displeasure and antagonism of a great many people, including some biologists themselves. In fact, American biologists know very little of Haeckel. They have heard that he is anti-religious, and they fight shy of his works. They are all Darwinians, but they know nothing about Haeckel. I wrote him that I had done what I could to make his work known to the American public, especially to the biologists, but that I did not flatter myself that my efforts had been attended with very great success. In America it is not the biologists who are the defenders and admirers of Haeckel, but the great enlightened lay public, who have no axes to grind, no specialties to cultivate, and who want the truth, the whole truth, and nothing but the truth. They are the ones who are doing the work, and they are the main dependence on which we must all rely for the recognition of our advanced work. I have cultivated two or three very narrow specialties, but I have entirely given up hope that specialists will ever be imbued with many of the great truths that science teaches. They are not in search of truth, but only of facts.

Now, the Thomas Paine movement is a political movement. There was no sociology then, as Mr. Wakeman has said. Mankind was then in the political struggle. Thanks to Paine and other great reformers, we have emerged from the condition where the political struggle is the main issue. In other words, political liberty has been attained. All the great advanced nations of the world are democracies. They may be nominal monarchies, but, with the exception of a few countries like Spain and Russia, they are virtual democracies. The next struggle that succeeded it was the economic struggle. There is another struggle that is very intimately associated with the economic one, and that is the great important struggle of to-day. I call it the social struggle. When men were in the political struggle they imagined that when their political rights should be attained the millennium would be here. But they found it was nothing of the kind; that they had not reached any such state, but that there was another great struggle to be gone through, the economico-social struggle, the struggle of to-day. The political struggle confined itself to the third estate rising and overthrowing the first and second estates (clergy and nobility). The struggle of to-day is in the direction of a contest for the attainment of social and economic equality, and is the effort on the part of the fourth estate, which used to be called the proletariat—the working classes, the mass of mankind, to secure social emancipation.

Returning to Haeckel for a moment, I want you all to understand that Haeckel, in addition to being a biologist and a philosopher is also in a true sense a sociologist. He has edited, and it is through his instrumentality that they have been sent to me, a series of works called "Nature and the State," consisting of eight volumes, all relating to sociology in its proper sense, published under his direction and supervision and distributed to the world. But that is not all. Haeckel's "Riddle of the Universe" and his "Wonders of Life" are works on sociology, to a very great extent. You will find in his "Wonders of Life" a most magnificent statement of the whole progress of human development, the best I have seen, in that it supplements Lewis H. Morgan's great work, bringing it down to the highest and latest stages of civilization and enlightenment. It even ventures to peer into the future.

So perhaps I have succeeded in avoiding the Scylla and Charybdis that confronted me when I stepped upon this platform, and have said all that I ought to take your time in saying about both Haeckel and sociology.

561. [Appreciation of: Sociological Study of the Bible, by Louis Wallis]

History.—Written February 1, 1912. Mr. Wallis's able articles on *Biblical Sociology* had been running through the *American Journal of Sociology* for two years or more, and I had read them with great interest. I saw that he not only stood for higher criticism, but that he seized the sociological aspects of the subject. I had had some correspondence with him, and he had called on me here at Brown University, when I introduced him to Professor Fowler who invited him to address his class in Biblical Literature and History, which he did. In January, 1912, he sent me a prospectus of the book to consist practically of the series of articles on *Biblical Sociology*, which he now proposed to call *Sociological Study of the Bible*, and asked me if I was willing to say a good word for it. His letter containing this request is dated January 27, 1912, and on February 1, I wrote him a somewhat extended letter which contained this passage. On April 15, I received the book. The wrapper or cover that came around the book gives the title on the back and sides, and on the front side are printed five complimentary statements including this of mine. That is the only sense in which this remark can be said to be published, but having been written by me and printed, it comes within the rule requiring its insertion at this place. There have been two other similar cases (see supra, Vol. IV, No. 325; Vol. VI, No. 468).

Your book is certainly the most considerable contribution thus far made to the sociology of the Hebrew race. You have shown that their history was a typical case of social assimilation, including all the stages and steps in that process, as described by Gumplowicz and Ratzenhofer.

EDITORIAL NOTE

The manuscript of the author ends here. Between the date of the preceding article and his death he wrote two articles both of which were published after his death. They are included as the final work of Lester F. Ward.

[*1913. Obit., April 18, 1913.*]

562. Définition du Progrès Social

History.—Written for the eighth congress of the International Institute of Sociology which was held at Rome in October, 1912. (See author's note on this article in No. 558.)

Annales de l'Institut International de Sociology, Tome XIV, Paris, 1913.

DÉFINITION DU PROGRES SOCIAL

Communication faite au 8^e^ Congrès de l'Institut International de Sociologie.

IL y a deux espèces de progrès, le progrès structural et le progrès moral. Le progrès structural se trouve non seulement dans la société, mais aussi dans les animaux et les plantes. On peut même dire qu'il se trouve dans le monde inorganique. Le progrès moral se borne à l'homme, ou, du moins, à l'homme et aux animaux. Par progrès moral on ne veut pas dire progrès dans la morale. Le monde moral est le monde du sentiment, et tout ce qui comprend le sentiment est moral. Le progrès moral comprend cette forme du sentiment seulement qu'on appelle le plaisir et la douleur. Tout ce qui augmente le plaisir ou diminue la douleur est progrès moral. Il se borne donc au monde animal. Dans cette étude je vais le limiter aussi à la race humaine. Puisque l'homme existe toujours dans la société, tout progrès moral devient progrès social. Il en est de même pour le progrès structural de la race humaine. Le progrès social est par conséquent de deux espèces, structural et moral. Le développement structural dans la société constitute la culture. On peut donc caractériser le progrés social structural comme progrès cultural, et les formes les plus élevées de la culture ont reçu le nom de civilisation.

Il y a longtemps j'ai défini la civilisation "comme l'utilisation des matériaux et des forces de la nature" (1), et j'ai répété cette définition plusieurs fois (2). Elle peut rester alors comme la définition du progrès social cultural. C'est "l'achèvement humain", ou "la conquête de la nature" (3).

Qu'est-ce donc, que le progrès social moral? Voici mes premières définitions, qui, quoiqu'un peu différentes de forme, sont les mêmes en substance: "Aucun progrès n'est réel qui ne montre pas une diminution de la totalité de la souffrance ou une augmentation de la totalité de la jouissance partout dans la société" (4). "Tout ce qui augmente la somme totale du bonheur humain"(5). "Augmentation du bonheur humain"(6). "Augmentation du bonheur humain, ou, négativement envisagé, diminution de la souffrance humaine" (7).

L'avancement principal que j'ai fait dans ces définitions se trouvait dans la conférence lue devant le Troisième Congrès de cet Institut en 1897, intitulée "L'Economie de la Douleur et l'Economie du Plaisir" (8). Dans cette conférence j'ai représenté progrès comme consistant dans un avancement d'une économie de la douleur vers une économie du plaisir. Sans doute l'idée essentielle est la même que dans les premières définitions, mais celle-ci reconnaît un vrai mouvement social dans les sens indiqué. Il n'y a aucun doute de la réalité d'un tel mouvement. En désignant comme *plus* les plaisirs, et comme *minus* les douleurs de l'existence, comme je l'ai fait dans cette conférence, nous pouvons définir le progrès social de nouveau comme *une augmentation de la somme algébrique du plaisir et de la douleur.* Il va sans dire que nous parlons ici du progrès moral seulement.

Comme je l'ai déja fait remarquer, le progrès structural dans la société est tout simplement le progrès de la civilisation, et j'ai toujours défini la civilisation comme "le résultat de l'utilisation des matériaux et des forces de la nature." Le degré énorme jusqu'où les matériaux et les forces de la nature ont été utilisés ne pouvait manquer d'augmenter les plaisirs et de diminuer les douleurs de l'existence. Cet effet se produit complètement pour tous ceux qui sont en état d'utiliser réellement ces matériaux et ces forces. Le fait que la civilisation ne produit pas le progrès moral tient exclusivement à la circonstance qu'un grand nombre des membres de la société sont exclus du pouvoir d'utiliser les matériaux et les forces de la nature. C'est le dérangement social qui cause ce résultat. L'utilisation des matériaux et des forces de la nature est l'achèvement humain, et ce qui n'a pas été accompli c'est "la socialisation de l'achèvement" (1). Lorsque tout achèvement humain aura été socialisé, de manière que tous les membres de la société partageront également ses bénéfices, la civilisation et le progrès reviendront à la même chose, et tout art remplira sa fonction normale d'augmenter la somme algébrique du bien et du mal dans le monde. La sociologie est la science qui pose les principes et indique la méthode pour atteindre cette fin.

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(1) *Dynamic Sociology*, vol. I., p. 26.

(2) Voir *The Psychic Factors of Civilization*, p. 189; *Outlines of Sociology*, pp. 186, 261; *Pure Sociology*, p. 255, 289, 467 (traduction française, *Sociologie Pure*, t. I, pp. 315, 360; t. II., p. 245).

(3) *Sociologie Pure*, chap. xix.

(4) *Dynamic Sociology*, vol. I., p. 67.

(5) *Ibid.*, vol. II., p. 161.

(6) *Ibid.*, p. 174.

(7) *The Psychic Factors of Civilization*, p. 287,

(8) Voir les *Annales* de cet Institut, t. IV., pp. 89–132.

LE PROGRÈS SPONTANÉ & LE PROGRÈS VOLONTAIRE

Communication faite au 8ᵉ Congrès de l'Institut International de Sociologie,

I.—PROGRÈS SPONTANÈ.

Le progrès spontané se trouve dans tous les domaines de la nature. Le progrès volontaire se borne à la race humaine, et en fait, à une très petite fraction des types humaines les plus avancés. Le progrès spontané dans le monde organique, au-dessous du niveau humain, et aussi dans la nature inorganique, est ce qu'on appelle plus ordinairement, et peut-être plus proprement, le développement ou l'évolution. Le progrès spontané humain, ou le progrès social, s'appelle aussi développement ou évolution, et la distinction entre le progrès spontané social et l'évolution sociale est difficile à reconnaître. C'est principalement le progrès social structural, mais il comprend quelque progrès social moral, comme j'ai défini ces termes dans ma communication précédente.

Personne n'a jamais nié l'existence du progrès spontané structural ou cultural, mais la prétention qu'il y a eu progrès spontané moral aussi a été souvent révoquée en doute.

a) Progrès spontané structural.

Les races, peuples et nations qui occupent la terre à l'époque présente manifestent pour la plupart des indications claires d'être devenus ce qu'ils sont par une suite de développement d'un état inférieur. Il semble probable que presque toutes les races, même les plus basses, se sont développées, et qu'elles montent toujours à des états de plus élevés, quoiqu'il soit difficile de l'observer dans les types les plus simples. Il y a, cependant, quelques moyens de déterminer ce fait sans la nécessité de l'observer directement.

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(1) *Sociologie Pure*, chap. xx.

La plupart des races inférieures ont changé leur système de famille dans une direction progressive. D'un état de mariage endogamique presque promisque, et généralement consanguin, elles ont adopté les formes de mariage qui ont empêché de plus en plus les unions consanguines. Plus tard, elles ont adopté un système exoganique qui défend absolument ces unions. Tout cela représente certainement le progrès, et c'est le progrès spontané parce que les races ont fait ces pas elles-mêmes.

Une deuxième preuve du progrès des races les plus inférieures dérive de l'archéologie et de l'étude de l'homme préhistorique. Une masse de faits indique que l'Europe, du moins, a été peuplée par une longue suite de races humaines devenant de plus en plus basses dans leur culture en remontant vers leur origine. En d'autres termes, l'archéologie nous montre qu'il y a eu développement, ou évolution, des races humaines qui ont occupé le territoire qu'on appelle l'Europe, par une série d'étapes ascendantes, depuis les hommes préglaciaux de Chelles, à travers l'âge paléolithique, jusqu'aux hommes post-glaciaux néolithiques de Robenhausen, et jusqu'à l'âge de bronze et de fer. Et cependant les hommes préhistoriques les plus avancés ne sont guère au niveau cultural des sauvages actuels les plus inférieurs. Il y a donc toute indication d'une condition générale de progrès dans l'humanité depuis son apparition comme descendante de quelque forme ancestrale animale; et cette transition de l'animalité à l'humanité peut être regardée comme le premier de ces pas dans son développement.

Si nous considérons les types hautement civilisés de l'humanité, les races dites "historiques," la même vérité se dégage. Les nationalités actuelles de l'Europe (les Anglais, les Français, les Allemands, etc.) étaient toutes barbares il y a deux ou trois mille ans. Et si nous regardons l'antiquité, la civilisation grecque et romaine, de six à dix siècles avant notre ère, il ne faut que remonter encore de quelques siècles pour les trouver dans la société dite "tribale" ou "gentile," justement en train d'émerger de l'état matronymique, tout en regardant l'Orient (Babylone, la Chaldée, l'Inde) comme la source de toute culture élevée. Mais il n'y a aucun doute que même ces civilisations plus anciennes, y compris l'Egypte, sont sorties d'un état de barbarie et même de sauvagerie. La Chine prétend à un maximum de 25.000 ans d'histoire écrite. Accordons même ceci, et comparons-le avec les 200.000 ou 300.000 ans que les géologues nous disent que l'homme a existé sur la terre. Il faut avouer que la période historique représente un avancement sur la période préhistorique, et c'est le progrès spontané humain.

Si nous bornons maintenant notre examen aux peuples les plus civilisés et les plus éclairés de la race caucasienne, et à la période historique, seule période pour laquelle nous avons l'habitude de parler du progrès, nous trouverons que la plus grande part de ce progrès a été spontanée. Il n'est guère nécessaire de signaler ce fait même, puisque c'est l'orgueil de notre âge. Le grand progrès industriel et scientifique des deux ou trois derniers siècles est le thème de tous ceux qui traitent de l'homme et de la société. Mais presque rien de tout cela n'est volontaire dans le sens que ceux qui l'ont accompli l'ont fait pour cet objet même. Pour eux, le progrès social est inconscient, non voulu, et non contemplé.

Il y a alors quelque chose dans la nature des choses et de l'homme qui cause le progrès partout et toujours. Les hommes de n'importe quelle race, de tout âge, et de tout pays, non seulement sont inconscients de l'effet social de leur action, mais ignorent qu'il y a progrès quelconque. Les races inférieures, comme tous ceux qui les ont étudiées le savent bien, sont excessivement conservatrices et ne conçoivent jamais un meilleur état que celui dans lequel elles vivent. Elles s'opposent vigoureusement à toute tentative pour changer cet état le moins du monde. Ce n'est que dans les races les plus développées que l'idée de l'amélioration ou du perfectionnement de la société se présente jamais. Dans le xviii^e^ siècle naquit une petite école de philosophes, typifiée en Allemagne par Herder et en France par Condorcet, qui parlait du progrès et proclamait la perfectibilité humaine. A l'arrivée de la révolution française, son nombre s'accrut, et on croyait que l'idée devait réellement prendre forme. Mais aprés la restauration des Bourbons, de bonne heure, dans le xix^e^ siècle, les défenseurs de cette doctrine diminuêrent et disparurent pour la plupart. Auguste Comte, malgré son système compliqué de régénération sociale, trouvait nécessaire de combattre la doctrine de perfectibilité.

Personne ne conteste ce fait qu'il y a eu progrès matériel, structural, ou cultural, ce qu'on appelle le progrès de la civilisation, parce qu'il est trop apparent, mais très peu de tentatives ont été faites pour l'expliquer. Cette tâche appartient à la sociologie, et, certes, elle vaut la peine de faire un effort pour l'accomplir. Mais c'est un problème beaucoup plus grand que celui d'expliquer le progrès relativement récent des arts dans le monde occidental. Le problème n'est autre chose que celui d'expliquer le progrès spontané structural de l'humanité dans tous les temps et sous toutes les circonstances. Cette tâche, je l'ai entreprise dans mon ouvrage "Sociologie Pure." Les lecteurs de cet ouvrage savent qu'il ne traite que les phénomènes sociaux spontanés. Le traitement de tous les phénomènes sociaux volontaires en est rigidement exclu. Cet ouvrage embrasse sans doute un champ beaucoup plus vaste que celui qui nous occupe à présent, mais dans le onzième chapitre, j'ai abordé ce sujet. En effet, j'ai anticipé cet ouvrage dans une conférence que j'ai faite devant le Quatrième Congrès de cet

Institut, intitulée "La Mécanique Sociale"(1). Vers la fin de cette conférence, j'ai posé un des plus importants des principes dynamiques qui produisent le progrès spontané de la société. Dans la "Sociologie Pure," je distingue trois de ces principes dynamiques, et je démontre que leur action réunie est adéquate pour expliquer à peu près tout le progrès social involontaire qui a été accompli.(2) Ces sources sont accessibles aux membres de l'Institut, et il serait superflu ici d'entreprendre une définition ou une explication de ces principes. Il nous importe seulement de saisir fortement la vérité que ce sont tous des principes inconscients, dans le sens que l'humanité, dans n'importe quel étage de la société, ignore le fait qu'il existe un progrès quelconque. L'homme n'a ni dessein, ni désir de changer l'état social, et il n'a aucune idée de l'avoir fait.

b) *Progrès spontané moral.*

Comme je l'ai déjà fait remarquer, on a souvent nié le fait du progrés spontané moral, et on a fait de grands efforts pour démontrer qu'il n'existe pas. La déclaration bien connue est que tout l'avancement fait par la société dans les arts de la vie et du plaisir n'a pas rendu l'homme plus heureux qu'il était avantque ces arts existassent. En d'autres termes, on affirme que les hommes civilisés ne sont pas en meilleur état au point de vue de la jouissance de la vie que les hommes non civilisés. On prétend même que les sauvages les plus bas sont plus heureux que les peuples les plus civilisés.

Je ne vais pas m'arrêter pour répéter ces déclarations bien connues qui remontent jusqu'à Rousseau et Fourier et reparaissent dans les discussions économiques modernes de Henry George et d'Emile Durkheim (3).

Pour démontrer leur fausseté, il ne faut que jeter un coup d'œil sur les principaux arts. Les grands besoins primaires de l'humanité sont la nourriture, les vêtements, le logement et le chauffage. Tous ces besoins, à l'exception du premier, dépendent exclusivement de l'art, tandis que la cueillette ne fournit que les espèces de nourriture les plus simples. Même la pêche, a l'exception du coquillage, exige quelque art et la chasse comporte l'existence de bien des arts. L'état pastoral est tout à fait artificiel et l'agriculture invoque les pouvoirs inventifs de l'homme à un très haut degré. Est-ce que ces états représentent un avancement quelconque sur la cueillette, ou les derniers sur les premiers, au point de vue moral? Est-ce que le pêcheur est plus heureux que celui qui fouille les racines, le chasseur que le pecheur, le berger que le chasseur, le fermier que le berger? Chacun s' élève sans doute un degré plus haut dans les satisfactions et les agréments physiques de la vie.

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(1) Voir les *Annales*, tome VII., pp. 163–203.
(2) Voir tome I., pp. 286–315.
(3) Division du travail social, p. 51.

Considérons maintenant la cuisine. Peut-on soutenir sérieusement l'idée que l'homme jouissait aussi complètement de son aliment à l'état cru qu'après avoir appris à le cuire? Presque aucun autre art n'a contribué autant à la jouissance de la vie que celui du cuisinier. Mais les autres arts par lesquels la nourriture devenait plus savoureuse, plus variée et plus saine, tendaient tous à augmenter la volume et la valeur de la vie.

Le vêtement, le logement et le chauffage sont, comme nous l'avons fait remarquer, tout à fait artificiels. Le seul effet qu'ils pouvaient avoir, c'était d'élever et de perfectionner l'existence. C'est-à-dire que leur effet est toujours entièrement moral. Cet état de choses continue à exister à un haut degré dans les conditions les plus civilisées de la société. La supériorité d'un peuple bien vêtu, bien logé et bien chauffé, sur un peuple presque nu, vivant dans des cavernes ou sous des tentes et sans feu, est une supériorité presque exclusivement morale.

On prend presque toujours les exemples dans une classe d'arts encore plus élevés, à savoir, les arts relativement modernes du transport et de la communication. Les sources principales dans lesquelles l'école des décadentistes puise ses preuves sont le chemin de fer, le bateau à vapeur, le télégraphe et le téléphone. Ces arts, nous dit-on, ne sont aucune amélioration sur la diligence, le bateau à voiles, le courrier, et le messager. Ils tendent à augumenter les inquiétudes et les tourments de la vie sans augmenter ses conforts et ses jouissances. Il n'y a aucune manière de prouver ni de réfuter ces assertions. Ce sont des déclarations creuses, gratuites et sans fond. La meilleure réponse qu'on puisse y faire, c'est de demander à ceux qui les font, s'ils voudraient retourner à ces modes primitifs de la vie. Que l'idée soit fausse, cependant, cela est évident à cause de l'engouement avec lequel tout le monde se sert de ces arts civilisés.

Jusqu'ici nous n'avons considéré que les avancements matériels dans la civilisation. Lorsque nous regardons le progrès psychique ou spirituel du monde, nous trouvons l'aspect moral beaucoup agrandi. Les types les plus inférieurs d'hommes sont les victimes de craintes perpétuelles. Leur crainte de la nature est beaucoup plus grande que celle des hommes civilisés qui ont appris à dompter et à utiliser la nature. Mais la source principale de la crainte parmi les races non civilisées est la crainte des êtres spirituels. La civilisation a l'effet d'écarter cette classe de terreurs et partant de libérer l'humanité de ce cauchemar terrible qui l'a toujours menacée. Cet effet a probablement plus accompli pour soulager les maux de l'homme et pour le rendre heureux que tous les arts matériels que nous avons énumérés.

Une autre forme du progrès spontané moral est ce que Tylor appelle les arts du plaisir. Dans leur état le plus élevé ils constituent les beaux-arts. Ceux qui nient le progrès moral du monde font rarement allusion à l'influence des beaux-arts. Quoiqu'on puisse regarder les beaux-arts en eux-mêmes comme des fins, ces fins sont seulement des fins morales. Les beaux-arts n'existent pas pour autre chose que pour contribuer aux plaisirs de la vie, et serait un homme hardi qui nierait qu'ils le font à un haut degré.

Nous voyons alors que le progrès spontané du monde a été progrès moral aussi bien que progrès cultural, et toujours et partout la somme algébrique du plaisir et de la douleur s'est agrandie. C'est le passage d'une économie de la douleur vers ou dans une économie du plaisir qui constitue le progrès moral.

II.—Progrès volontaire.

Il est évident que presque tout le progrès du monde, le progrès cultural non moins que le progrès moral, a été spontané et que l'homme n'a pas été conscient qu'il avait lieu. Y a-t-il donc quelque chose qu'on peut nommer progrès volontaire, c'est-à-dire progrès accomplir par l'action consciente dirigée vers cette fin?

Comme je l'ai indiqué dans le mémoire déjà cité (1), et plus amplement dans la "Sociologie Pure" (2), le seul effet dynamique de l'action individuelle est celui de transformer le milieu avec l'objet de satisfaire le désir. C'est cette transformation du milieu qui, à l'insu de l'agent, cause la plupart du progrès spontané de la société. Mais j'ai indiqué aussi (3) que dans toute société il a existé une classe oisive, exempte de la lutte pour l'existence, dont une certaine proportion a consacré ses énergies au travail progressif. La plus grande partie de tout le progrès intellectuel du monde a été redevable à cette classe. Or, cette classe, n'étant pas obligée de travailler pour les nécessités physiques, a travaillé seulement pour la satisfaction que donne l'exercice de ces facultés supérieures. Il n'y a aucun doute qu'un des principaux motifs pour un tel travail a toujours été l'idée de faire du bien à l'humanité. Personne ne conteste que cette classe a énormément avancé la civilisation. Y a-t-il une raison quelconque pour ne pas classer tout cela comme progrès volontaire? Je n'en vois pas. En effet, la plus grande partie des inventions utiles et des découvertes scientifiques a été faite par des motifs analogues. Les agents ne cherchaient aucune fin égoïste. Le seul désir qu'ils satisfaisaient était le désir de réussir. Le plaisir ou la satisfaction consistait dans l'exercice de ces hautes facultés. Qu'ils avaient réellement l'intention de faire du bien à l'homme par ce travail ne semble pas douteux, et beaucoup faisaient de grands sacrifices dans ce but. Les œuvres d'un Copernic, d'un Galilée, d'un Galvani, d'un Newton ou d'un Darwin n'étaient pas égoïstes, et le progrès accompli par eux est le progrès volontaire.

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(1) *Annales de l'Institut International de Sociologie*, tome VII., pp. 193-203.
(2) Tome I., pp. 306–314.
(3) *Op. cit.*, tome II., pp. 288–296.

Quand on discute le sujet du progrès volontaire on ne pense pas ordinairement au progrès fait par l'individu. On pense d'habitude à l'action de la société seulement, et des tentatives ont été faites pour démontrer que la société entreprend en effet et accomplit heureusement les améliorations de sa condition.

Le progrès volontaire social signifie donc: progrès qui a été fait sciemment par la société et avec cette fin en vue. Mais, qu'est-ce que la société dans ce sens d'un agent conscient et volontaire qui cherche son propre intérêt? Presque tous les sociologues regardent la société comme une organisation. Ce qui est organisé dans la société ce sont les multiples institutions humaines qui la constituent. Ces institutions sont aussi des organisations et se composent, à leur tour, d'individus. Beaucoup d'institutions, cependant, sont aussi des organisations composées d'institutions d'ordres inférieurs. La société est ainsi une hiérarchie d'organisations subordonnées. L'individu est simplement l'unité primaire de la série. Les organisations qui constituent la société appartiennent à deux classes, les volontaires et les involontaires. L'adhésion aux organisations volontaires est facultative, aux organisations involontaires, obligatoire. Ces dernières embrassent tous les membres de la société, et la plus haute forme en est l'Etat. Néanmoins, il y a beaucoup d'Etats, ou d'autonomies politiques distinctes, et l'action de la société consiste ordinairement dans l'action de l'Etat. L'action des organisations politiques subordonnées constituerait aussi, cependant, une action sociale, comme, par exemple, celle des municipalités.

Or, on suppose l'individu incapable d'action sociale. N'importe combien il contribue aux progrès sociaux, son action appartient à la classe inconsciente et spontanée déjà décrite. Il en est de même pour la plupart des organisations volontaires, surtout pour celles des affaires. Il y a, cependant, des organisations de charité qui font un service social et contribuent quelquefois au progrès social. La plus grande part de l'action politique se rapporte à la défense et à la protection de l'Etat, et elle ne comporte pas le progrès social. Néanmoins, au fur et à mesure que les Etats deviennent éclairés, la disposition à entreprendre des mesures progressives s'accroit. Quoique bornée aux limites territoriales de chaque Etat particulier, cette espèce d'action constitue le progrès social. Le mouvement moderne vers la conservation des ressources naturelles des différents pays est un exemple typique d'action sociale dans l'intérêt du progrès. L'établissement des institutions de charité par les Etats, les villes, etc., quoiqu'on ne puisse pas dire qu'il constitue le progrès social, a du moins le caractère du désintéressement, et diffère partant de beaucoup de l'activité individuelle égoïste. Les systèmes d'instruction publique sont les cas les plus typiques que nous ayons du progrès social volontaire. En les introduisant, la société semble s'apercevoir qu'elle ouvre la voie pour la génération prochaine de ses membres.

Il n'y a pas de doute que parmi les peuples éclairés, la société, par ces actes et par d'autres actes analogues, devient de plus en plus consciente de son pouvoir de s'élever, et avant longtemps probablement, des mesures progressives seront entreprises sur une échelle beaucoup plus grande que jamais jusqu'ici. Non seulement on demandera la conservation des ressources naturelles contre les gaspillages de l'égoïsme individuel, mais on entamera des opérations étendues pour la préservation de la santé et pour la jouissance de la vie.

Le progrès volontaire de l'avenir sera cultural et moral en même temps. Au lieu d'être structural sans être moral, comme dans une si grande partie du progrès spontané du monde, il sera principalement moral, et chaque nouvelle structure sera spécialement adaptée pour l'augmentation de la somme algébrique du bien et du mal.

[563.] Eugenics, Euthenics, and Eudemics

History.—This article was prepared as a lecture and was delivered before the Federation for Child Study in New York, on January 30, 1913.

The American Journal of Sociology, Chicago, Vol. XVIII, No. 6, May, 1913, pp. 737–754.

EUGENICS, EUTHENICS, AND EUDEMICS[1]

EUGENICS

ARTIFICIAL SELECTION

WHEN Charles Darwin, in his great work on *Variations of Animals and Plants under Domestication*, published in 1868, taught the whole world the marvelous efficiency of artificial selection, it was no wonder that the idea of applying it to the human race should have occurred to many. His talented cousin, Francis Galton, was the first publicly to suggest such an application. He had used the word "stirp"[2] for the primary bearers of heredity, and he perceived that in the improvement of cereals and fruit trees, and the breeding of domestic animals, man had been engaged practically in the culture of hereditary stirps.

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[1] A lecture delivered before the Federation for Child Study in New York, on January 30, 1913.

[2] In a paper entitled, "A Theory of Heredity," read before the Anthropological Institute of Great Britain, November 9, 1875, and published earliest in the *Contemporary Review* for December, 1875, where the word is used and explained on p. 81, and soon after in the *Journal of the Institute*, V., No. 3 (January, 1876), where the same passage occurs on p. 330.

STIRPICULTURE

In an article on "Hereditary Improvement," published in *Fraser's Magazine* for January, 1873, he had used the word "viriculture" (p. 119) for what he now renamed "stirpiculture," and his claim that mankind might be made the beneficiary of this potent principle seemed altogether reasonable. But such a captivating idea could not fail to be seized upon by charlatans and carried to unwarrantable lengths, and very soon the term "stirpiculture" had degenerated and become objectionable to all refined natures. Galton was therefore compelled to abandon it and to adopt another which could not be so easily prostituted to coarse sensual ends, and in 1883 he introduced the word "eugenics"[1] for practically the same idea. This term has been kept fairly within the pale of science, but it has almost set the world on fire, and now seems to engross the attention of all classes. Many see in eugenics the regeneration of mankind. Is there a rational basis for this, or is it only a temporary popular "craze," doomed to collapse after a short period?

NEGATIVE AND POSITIVE EUGENICS

Dr. C. W. Saleeby[2] has clearly pointed out that eugenics embraces two quite different fields, which he very appropriately calls negative eugenics and positive eugenics, respectively. Negative eugenics relates to the problem of preventing the mental and physical defectives of society from perpetuating their defects through propagation. Positive eugenics relates to the problem of improving the mass of mankind by the selection of the superior in the process of reproduction.

It is clear that these are two entirely distinct problems. As the defectives are the wards of society, society has somewhat the same control over them as intelligent men have over the vegetable and animal kingdoms, and there is no good reason why it should not act in the same way with regard to them, and eliminate as completely and as rapidly as possible the worthless elements in the population. Nothing but an inexcusable indifference, due to general ignorance and the half-unconscious state of society, prevents this being done. This problem has been adequately treated by the two lecturers who have preceded me on this platform, and I will therefore make no attempt to treat it myself.

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[1] In his book entitled, *Inquiries into Human Faculty and its Development*, London, 1883, p. 44. He here says: "The investigation of human eugenics—that is, of the conditions under which men of a high type are produced—is at present extremely hampered by the want of full family histories, both medical and general, extending over three or four generations." He does not even italicize the word here, and it is evident that in thus incidentally using it he had no idea of the rôle that it was soon destined to play.

[2] *Sociological Review* (London), II. (July, 1909), 228.

POSITIVE EUGENICS

The great problem remains, the problem of positive eugenics. The principle underlying it is beyond question. The power of an intelligent being to modify to almost any extent the qualities of organisms over which such a being has complete control has been demonstrated beyond a peradventure. Why, then, may not human nature be thus modified, and the human race be lifted up in the same way and to the same degree that the vegetable and animal races have been lifted up? In 1891 I myself said[1]:

> Artificial selection has given to man the most that he possesses of value in the organic products of the earth. May not men and women be selected as well as sheep and horses? From the great stirp of humanity with all its multiplied ancestral plasms—some very poor, some mediocre, some merely indifferent, a goodly number ranging from middling to fair, only a comparatively few very good, with an occasional crystal of the first water—from all this, why may we not learn to select on some broad and comprehensive plan with a view to a general building-up and rounding-out of the race of human beings? At least we should by a rigid selection stamp out of the future all the wholly unworthy elements. Public sentiment should be created in this direction, and when the day comes that society shall be as profoundly shocked at the crime of perpetuating the least taint of hereditary disease, insanity, or other serious defect as it now is at the comparatively harmless crime of incest, the way to practical and successful stirpiculture[2] will have already been found.

Indeed it seemed at first very simple, but the moment a practical application of it is contemplated an insuperable difficulty arises. The control of heredity is possible only to a master-creature. Man is the master-creature of the animal world. Society is the master of its defectives. But normal people are their own masters. History tells of sumptuary laws by which kings controlled the food and clothing of their subjects. This has all been long since done away. But the most extreme sumptuary law would be liberty itself compared with any attempt on the part of society to control the choice of partners in their marital relations. This would be a tyranny by the side of which all other tyrannies would fade into insignificance.

So clear had this become to Galton himself that he had long confined the practical teaching of positive eugenics to the idea of making the laws of heredity so widely and thoroughly known that people would practice them for themselves, and thus automatically, as it were, perfect the human race. That this is possible no one will question, but there are reasons why its success must remain comparatively small even in the most enlightened communities.

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[1] "Neo-Darwinism and Neo-Lamarckism," annual address of the president of the Biological Society of Washington, delivered January 24, 1891, *Proceedings*, VI., 71.

[2] I had not at this date heard the word "eugenics," but the word "stirpiculture" was quite common in America. In fact, even in England, it was supposed to be an American word. Thus, Mr. H. G. Wells, in discussing Galton's paper on "Eugenics," read before the Sociological Society of London on May 16, 1904, said: "'Eugenics,' which is really only a new word for the popular American term 'stirpiculture,' seems to me to be a term that is not without its misleading implications." See the report of that meeting published in the *American Journal of Sociology* for July, 1904, X., 11. To this remark of Mr. Wells, Mr. Galton replied (*ibid.*, pp. 24–25.): "Mr. Wells spoke of 'stirpiculture' as a term preferable to 'eugenics.' I myself invented it, and deliberately changed it for 'eugenics.'"

In the first place, eugenists are not usually willing to admit the enormous force of personal preference. When two persons are attracted to each other by those subtle laws of their emotional natures, they are not likely to inquire whether they are fitted to maintain the standard or improve the quality of the race. However deeply grounded in the laws of heredity they may be, their passion completely blinds them to all lack of harmony with such laws. Even if they attend to it, they are sure to believe that no such deficiencies exist. We cannot imagine them abandoning their intentions on that account. The number of cases in which this would occur would be so extremely small as to produce no appreciable effect upon society or the race.

In the second place, Galton was not willing to accept the popular view that Nature herself tends to bring about race improvement through the selection of opposites. He even attempted to prove from a few statistical facts, taken from the highest classes where exceptions would be most likely to occur, that the reverse was the case, and that the sexes preferred similar, and not different, qualities.

The biological imperative.—There are many ways in which Nature strives to maintain a perfect race, and even to improve it. I have grouped all these tendencies together under the phrase "biological imperative,"[1] and it constitutes one of the most salutary principles of sociology. Moreover, it is not recognized or understood by eugenists, which is a serious defect in their doctrine. It is the *vis medicatrix naturae* of society. A large part of the degeneracy of the higher classes is due to the neglect of this principle, and to the attempt, often successful, to defeat its normal operation. There has been too much interference with Nature's ways. Man assumes to know better than Nature how to guide the forces of heredity. He sets up artificial imperatives—the social imperative, the categorical imperative—and he thereby thwarts Nature in her wholesome tendencies, which all look to the vigor of the race. It is these manifold social and artificial restraints that are bringing about race degeneracy and social decadence. There is serious danger that the teachers of eugenics may take a false road, and, in so far as they can influence human selection, may work deterioration rather than amelioration.

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[1] See *Pure Sociology*, p. 302.

In the first place, eugenics tends to emphasize unduly the intellectual qualities. Galton's whole interest was centered on hereditary genius. The only kind of superiority generally recognized is intellectual superiority. The only organ that it is sought to improve is the brain. If the eugenists could carry out their plans the human head would be enormously enlarged at the expense of the rest of the body. This would soon make it impossible for men to be born, for obstetricians know that the head is now so unnaturally large that birth is a great hardship for woman.

In the second place, eugenists manifest more or less contempt for the affective faculties. The emotional side of man's nature becomes for them mere sentimentality. Brains, intellect, genius, alone have worth. Like the breeders of cattle, they would "breed for points," and the head is the only organ that they seek to develop. But Nature is far wiser, and seeks to develop all the faculties and to prevent all extremes. The normal becomes the ideal. A perfect race is one that is developed in all its qualities, physical, moral, and intellectual. It is this that the biological imperative aims at, and if not interfered with by the doctrinaires of heredity, this will be the result.

THE WELL BORN

It is strange that in the science of the well born all emphasis is laid on the ill born. To read the eugenic literature one would infer that the majority of mankind are defectives. The United States Census shows that the number of mentally defective (insane and feeble-minded) is about 333 per 100,000, or one-third of 1 per cent. While of course allowance must be made for errors in the statistics, and while there are many weak-minded persons who fail to be classed among defectives, still, after all corrections are made, the number of defectives cannot exceed one-half of 1 per cent. of the population, leaving 99.5 per cent. for the normal classes.

Galton talked as though it were possible to arrange all the members of society in a series extending from idiocy to genius, and he supposed that the resultant figure would show a regular gradation, such as Quetelet found in measuring the stature of Belgian soldiers, by which he arrived at his well-known *homme moyen*, which some facetiously translate, the "mean man." With Galton the defectives on the one hand and the geniuses on the other represent mere "deviations from a mean." That "mean" is the great mass, or mediocrity. In his *Hereditary Genius* (p. 24) he gives a figure of Quetelet's results, indicating the deviations by lighter shading. He does not give any figure for mental deviations, but they might be represented by two triangles with a common base and with the apices opposite each other, as in Fig. 1.

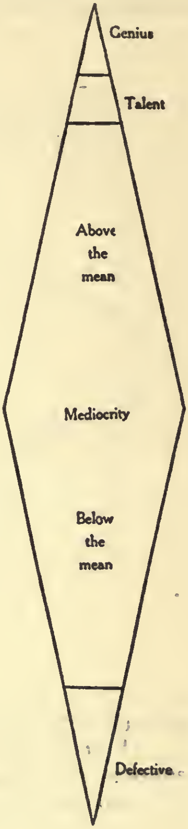

FIG. 1

This view is shared by Ribot and many others, and may be regarded as the eugenic doctrine. Ammon[1] showed that it is not true for mental phenomena, and that the normal condition includes the bulk of the population. On p. 53 of the first edition, and p. 173 of the second, of his work on the *Social Order*, he gives a figure illustrating this, which, with the omission of some details, I have reproduced (Fig. 2).

This is much nearer to the real state of things, but still greatly exaggerates the mentally defective. The geniuses are probably also exaggerated, because the existing state of society does not enable the mental powers to show themselves.[2] There is scarcely any gradation from the insane and feeble-minded to the normal condition, and the transition should be represented as abrupt. Much, too, that is called genius is pathologic, and belongs rather at the bottom along with the insane. A figure like that below (Fig. 3) would come much nearer the truth.

But it is scarcely worth while to try to indicate the amount of genius. We really know nothing about it. All the estimates are based on the actual number who have accomplished something in the world, but this, as I have abundantly shown, is no criterion of the amount of real genius, because the greater part of it lies latent in the great mass, and has never had an opportunity to manifest itself. The amount of visible genius has never exceeded one-tenth of 1 per cent., but it is proved that at least two hundred times as much exists and might be brought out. This would raise it to 20 per cent. But when we recognize the many forms that genius takes we cannot escape the conclusion that some measure of genius exists in nearly everyone. All this genius is scattered somewhat uniformly through the whole mass of the population.

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[1] Otto Ammon, *Die Gesellschaftsordnung und ihre natürlichen Grundlagen*, Jena, 1895; zweite verbesserte und vermehrte Auflage, 1896.

[2] See *Applied Sociology*, chap. ix.

Negative eugenics aims at the elimination of the defective element. Positive eugenics seeks to increase the number of geniuses. This can be done only from the ranks of the normal, *i. e.*, of mediocrity. These, and also the superior, and even the geniuses, are supposed to be capable of almost indefinite intellectual improvement through hereditary selection. This is at least a recognition of the existence of potential genius.

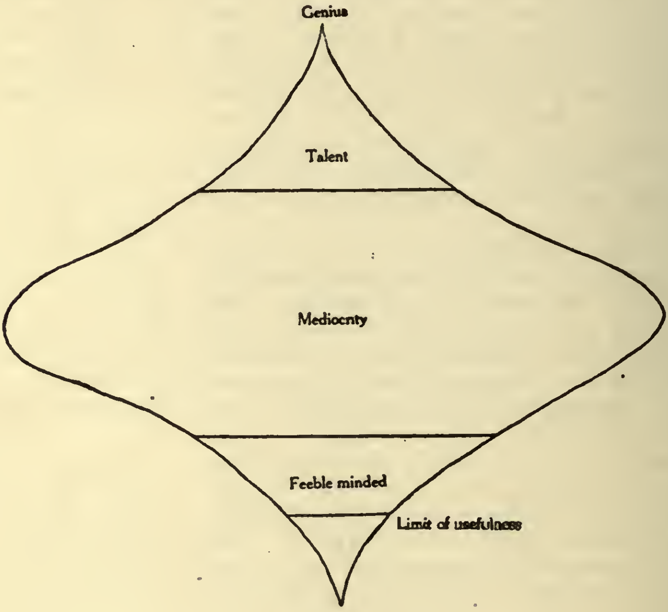

FIG. 2

THE LIFE-IMPULSE

The greatest problem before the world to-day is that of a universal upward tendency in nature. One aspect of it has been called "vitalism," or "neo-vitalism." But it is not less chemism and psychism. The biologists are alarmed at it. They see in it the specter of the old metaphysical life-entity. They are frightened at the bugbear of a life-god. But the discussions show that they fail to understand the principle. Properly conceived, it is entirely scientific, and has nothing to do with Reinke's "dominants" or Driesch's "entelechy" or "psychoid." Bergson talks of an *élan vital*, leaving it doubtful, however, as to what he really means. But the principle is not new. It has been dimly seen by many truly great philosophers. It has also been perceived by men of science. The eminent botanist Nägeli[1] clearly

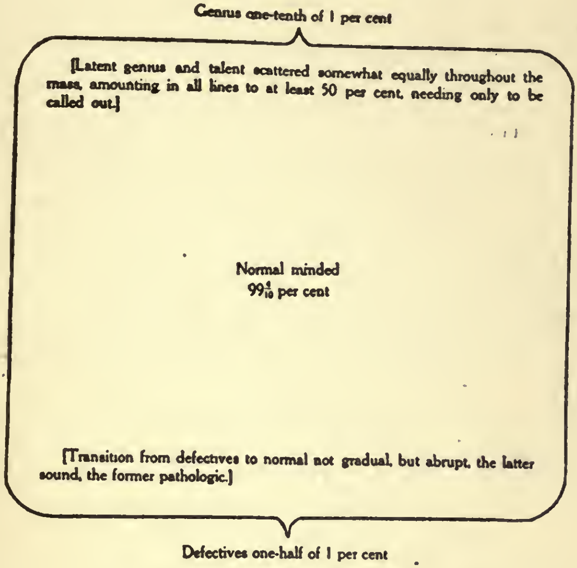
Genius one-tenth of 1 per cent
[Latent genius and talent scattered somewhat equally throughout the mass, amounting in all lines to at least 50 per cent, needing only to be called out.]
Normal minded
$99\frac{4}{10}$ per cent
[Transition from defectives to normal not gradual, but abrupt, the latter sound, the former pathologic.]
Defectives one-half of 1 per cent

FIG. 3

saw it in the plant world, and set it forth as the basis of his system. The great American botanist, Dr. Asa Gray, who was also a leading expounder of Darwin's entire system, accepted this view, and says that it was shared by another great European botanist, Alexander Braun. Criticizing the view that variation has no definite course, but takes place in all directions alike, he says: "What we observe in the seed-bed does not suggest this view. Nägeli, Braun, and myself incline to the opinion that each plant has an inherent tendency to variation in certain general directions."[2] The fact is that it is always in an upward direction, toward higher structural perfection.

What, then, is this principle? At bottom it is the universal energy at work in all nature. We have only to recognize that this universal energy is constructive and creative, as I have so fully shown.[1] It builds, and to it are due all the manifold forms of existence—worlds, atoms, organisms, man, society. Evolution is a series, and it is also an ascending series. The "nisus of nature"[2] is constantly pushing higher and higher structures into existence. The history of our earth is the history of a series of ascending steps in organic evolution. Each geologic age reveals the presence of higher types of both plants and animals than the preceding age. The present flora and fauna of the globe represent the last stage thus far attained in this ascending series. Toward the end of the animal line man emerged as the crown of the organic process. But man himself also constitutes an ascending series, and his history, unaided by the efforts of the eugenists, is one of progress from the lowest nature—men, to the highest culture—races. This prolonged spontaneous upward movement of the entire organic world is the result of that form of the universal energy which inheres in the life-principle, and which makes life a progressive agent, mounting "through all the spires of form."

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[1] Carl Nägeli, *Entstehung und Begriff der naturhistorischen Art*, zweite Auflage, München, 1865; *Mechanisch-physiologische Theorie der Abstammungslehre*, Leipzig, 1884.

[2] *Structural Botany*, New York and Chicago, 1879, p. 319.

The present eugenic movement is one of distrust of nature, of lack of faith in great principles, of feverish haste to improve the world, of egotism in the assumption of a wisdom superior to that of nature. If it could have its way it would thwart and distort the spontaneous upward movement, and create an artificial race of hydrocephalous pigmies. Fortunately its power is limited, and can produce only a ripple on the surface of society.

EUTHENICS[3]

Is there, then, nothing to do? Are we to accept that modern scientific fatalism known as *laissez faire*, which enjoins the folding of the arms? Are we to preach a gospel of inaction? I for one certainly am not content to do so, and I believe that nothing I have thus far said is inconsistent with the most vigorous action, and that in the direction of the betterment of the human race. The end and aim of the eugenists cannot be reproached. The race is far from perfect. Its condition is deplorable. Its improvement is entirely feasible, and in the highest degree desirable. Nor do I refer merely to economic conditions, to the poverty and misery of the disinherited classes. The intellectual state of the world is deplorable, and its improvement is clearly within the reach of society itself. It is therefore a question of method rather than of principle that concerns us.

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[1] *Pure Sociology*, pp. 79, 171.

[2] *Dynamic Sociology*, II, 93, 178; *Pure Sociology*, pp. 22, 136.

[3] *Euthenics. The Science of Controllable Environment. A Plea for Better Living Conditions as a First Step Toward Higher Human Efficiency.* By Ellen H. Richards, Boston, 1910.

Mrs. Richards gives (p. vii) the etymology of the word and its classic uses. The central thought seems to be that of material prosperity. I shall use it as embracing practically all that Galton meant by "nurture."

THE HUMAN BRAIN

The brain of primitive man was so large and so highly developed that he reasoned himself into all sorts of errors, which he has handed down to us, and which have constituted the most serious impediment to the progress of the world. Cephalization, natural selection, and especially sexual selection, co-operated during a long prehuman period to give to man a head and a brain much too large for his good, and which has been for the most part a biologically disadvantageous character. It is only coming to serve a useful purpose in the most advanced stages of civilization. Brain increase is therefore not at all the needed thing for the improvement of the race. The chief evils of the world have been due to error, which is a product of reason. Error is in turn the result of the ignorance of man of his environment, and his consequent false reasoning about it. The progress of the world has been due to scientific investigation, by which the true nature of the environment has been made known and the error removed. The thing needed for the improvement of the race is therefore more knowledge and not more brains.

COMPETITION

Everywhere in nature there is a tendency for the avenues of progress to become choked and the normally upward movement checked or arrested. The rising tide of world-progress, of which I have spoken, has therefore always been rhythmic. A dynamic state is converted into a static one, and kinetic activity into stagnation. The world has its resting stages, waiting for some new influence to enter in and produce a difference of potential. In the organic world competition tends constantly to keep the organism far below its possibilities of development. In the struggle for existence every organism that survives is arrested on a comparatively low plane. The survival of the fittest is only the survival of low types adapted to a hostile environment. They are all striving to rise to higher stages under the spur of that inherent force, or life-urge, which is everywhere and always pushing upward toward higher and better things. Nature is literally "bound fast in fate" by the competitive forces everywhere at work. Every plant and every animal possesses potential qualities far higher than its environment will allow it to manifest.[1]

The signal success of artificial selection is due entirely to this principle. The truth, however, is ignored by those who avail themselves of the principle. It is ignored by eugenists, who imagine they are adding something to the native powers of men, when all they can do at most is to loose the fetters with which nature has bound them. All that the agriculturist or the horticulturist can do, all that he needs to do, is to remove the hostile influences that restrain the native energies of the vegetable kingdom, and permit those energies to lift them to higher levels of existence. All that the stock-breeder can do is to liberate certain selected parts of the animal organism, and permit those parts to expand by their inherent powers. All that the eugenist could do, if he had full power to transform human beings in conformity with his capricious notions of what constitutes improvement, would be to set free the particular elements of human nature that he should select, and watch the workings of those potential agencies that had been hitherto cramped into quiescence.

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[1] I set forth this principle and established it by numerous examples as long ago as 1876. See my article on "The Local Distribution of Plants and the Theory of Adaptation" in the *Popular Science Monthly* for October, 1876, IX, 676–84.

But all this is *nurture*, pure and simple. Nature is unchanged. The hereditary tendencies remain the same. These are beyond the reach of human art. It is the environment that holds the hereditary impulse down. Man has no power over heredity. The only thing he can affect is the environment. It is true that man can utilize the laws of heredity. But the utilization of any law of nature consists simply in so adjusting the environment that the law shall operate in his interest. It is merely directing the forces of nature into channels of human advantage. But those channels belong to the environment.

SPONTANEOUS VARIATION

Darwin recognized the existence of spontaneous variation. In fact, he admitted that it must be called in to explain the first step in natural selection. Without it there would be nothing for natural selection to lay hold of. Enamored of the great principle of natural selection, biologists have fallen under the illusion that it explains everything. But spontaneous variation is a more fundamental principle. It is prior in order, and it is universal in nature. It goes on along with natural selection and in perfect harmony with it. It explains great numbers of facts that natural selection is powerless to explain. This latter can explain the biologically advantageous only, while those changes which are devoid of utility can be explained by spontaneous variation only. I long ago emphasized this fact and illustrated it by striking examples from my own special field.[1] I called it "fortuitous variation," but the phrase was due to the impression under which I then labored that Darwin himself had used it. But it is the same as his "spontaneous variation," simply emphasizing the fact, which so strongly appealed to me, that such variations take place, as it were, by chance, and not because they are useful. In point of fact, as I have repeatedly shown, they are due to that inherent impulse of the whole organic world, which is perpetually pressing in all directions, and striving to lift all life to higher levels, and which really brings about organic evolution.

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[1] See a brief abstract, all that was ever published, of my paper on "Fortuitous Variation in the Genus Eupatorium," in *Nature*, London, for July 25, 1889, XI, 310. On account of the importance of the idea, even for sociology, and the completely buried condition of this note, I reproduced the essential part of it in *Pure Sociology*. See pp. 241–42.

THE ENVIRONMENT

It turns out, then, that after all the discussion of heredity, and the hopes hung upon the idea of utilizing it in the interest of race improvement, it is a fixed quantity which no human power can change, while the environment, which Galton affected to despise, is not only easily modified, but is in reality the only thing that is modified in the process of artificial selection, which is the essential principle of eugenics itself. All the improvement that can be brought about through any of the applications of that art must be the result of nurture, and cannot be due to any change in nature, since nature is incapable of change.

There is a sense in which the environment may be regarded as representing opposition.[1] It is the environment in the widest sense that resists the upward pressure of the life-force, and holds all nature down. That force is like an elastic spring coiled up beneath a mass of environmental débris, and needing only to be freed in order to unfold spontaneously and lift the organic world to higher and ever-higher planes. In the human field the mind-force is added to the life-force, and both vital and psychic powers press forward together toward some exalted goal. The environment lies across the path of both and obstructs their rise. The problem everywhere is how to unlock these prison doors and set free the innate forces of nature.

THE ORGANIC ENVIRONMENT

Darwin has taught us that the chief barrier to the advance of any species of plants or animals is its competition with other plants and animals that contest the same ground. And therefore the fiercest opponents of any species are the members of the same species which demand the same elements of subsistence. Hence the chief form of relief in the organic world consists in the thinning out of competitors. Any species of animals or plants left free to propagate at its normal rate would overrun the earth in a short time and leave no room for any other species. Any species that is sufficiently vigorous to resist its organic environment will crowd out all others and monopolize the earth. If nature permitted this there could be no variety, but only one monotonous aspect devoid of interest or beauty. Whatever we may think of the harsh method by which this is prevented, we cannot regret that it is prevented, and that we have a world of variety, interest, and aesthetic attractiveness.

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[1] See *Applied Sociology*, pp. 123–28, 233–34.

THE SOCIAL ENVIRONMENT

It is not generally realized that in this respect the social environment does not essentially differ from the organic environment. It is true that Malthus taught us this more than a century ago, but we go on deploring the action of this law in the human race, and striving to nullify it by all manner of artificial devices. That rational man has the power through his intelligence to rob this law of its harsh, painful features is beyond question, but whenever this is attempted there is a general outcry against it, and those who attempt it are accused of an unpardonable sin against nature, and usually of a sin against God.

RACE SUICIDE

On April 12, 1901, Dr. Edward A. Ross, in his annual address before the American Academy of Political and Social Science in Philadelphia, speaking of the displacement of the American by the foreign population in this country, uttered these words:

> For a case like this I can find no words so apt as "race suicide."[1] There is no bloodshed, no violence, no assault of the race that waxes upon the race that wanes. The higher race quietly and unmurmuringly eliminates itself rather than endure individually the bitter competition it has failed to ward off by collective action.[2]

The phrase "race suicide" was immediately taken up and echoed throughout the civilized world. "Race suicide" was loudly and widely condemned, and branded as a mark of decadence. Chief among those to make a public use of it and indulge in its wholesale condemnation was Theodore Roosevelt, who lost no opportunity to weave it into his speeches and warn his audiences against its insidious dangers to mankind. He was credited with the authorship of the phrase, and the press took it up and scattered it broadcast over the world. Books with it as their title have been written, and the literature of foreign countries is now replete with translations of it into all languages.

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[1] The use of quotation marks here was probably not intended by Dr. Ross, as the words were not quoted, and were here used for the first time.

[2] *Annals of the American Academy of Political and Social Science*, Philadelphia, XVIII (July, 1901), 88.

THE FALLING BIRTH-RATE

All this is simply the latest aspect of the general alarm that has come into the world at the manifest decline in the birth-rate of civilized countries. Many see in it the approach of the end of the human race itself, and are filled with all manner of sinister forebodings. In France, where it is most extreme, and where a stationary condition of the population has almost been reached, thorough scientific inquiries into the causes have been instituted by the government, with the general result of establishing the fact that the limitation of families is in the main voluntary and purposeful. There are many elements that must be considered in the complete understanding of the problem, but the great truth stands clearly forth that people are no longer willing to contribute to the population at the rate prescribed by nature.

THE LAW OF POPULATION

Further investigations in all countries have shown that the movement is general, and although the problem is complicated by a large number of special influences, there comes forth from amid it all a great law, which may be called *the law of population*. It is very distinct from Malthus' "principle of population," and may be stated in this simple form: *population is inversely proportional to intelligence*.

What is the meaning of this law? It means that man, in proportion to his intelligence, is learning to mitigate the cruel law of the organic environment, which consists, as already stated, in reproducing greatly in excess of the possibility of existence, and then killing off the surplus. By whatever name we may call it, this law has in fact applied to mankind in all the ages past. It still applies to the uncivilized races, and it no less applies to the lower classes of civilized society. These still remain prolific. They furnish the *proles*, and constitute the proletariat. But nature knows how to keep down population, and they are still the victims of the organic law. And in so far as the population of civilized countries is permitted to increase, it must be from the proletariat. The enlightened classes refuse longer to furnish soldiers to gratify the ambition of military chieftains. They seek comfort and happiness, and have learned how to obtain them. They prefer quality to quantity, and demand *multum non multa*. They are accomplishing the same end as nature, viz., numerical uniformity, but they are doing it without destruction and without pain. Their remedial agent is a prophylactic. They have solved the Malthusian problem by the discovery of restraints to population of which Malthus never dreamed.

EUDEMICS.[1]

There are many who look with alarm at the fact that population is being to so great an extent recruited from the base, *i. e.*, from the lower classes. Such apprehensions are due to the almost universal error that those classes are inferior to the middle and higher classes. This is not the place to refute this error, and I have done it elsewhere,[2] but could it be removed, all grounds for alarm would be dispelled. If there are signs of decadence anywhere they are not in the proletariat. They are to be found among the pampered rich and not among the hampered poor. These, though ill bred, are well born; their infusion into the population imparts to it a healthy tone. It constitutes the hope of society.

On a former occasion[3] I emphasized this fact in language which I could not now improve, and which, therefore, as a concluding word, I will ask permission to repeat:

> The paper of this morning treats the problem to which Galton, Karl Pearson, Ribot, Lombroso, Ferri, and many others have devoted so much attention—namely, the physiological improvement of the race of men. One aspect of that problem was thoroughly discussed yesterday, and it is remarkable that neither the able paper of Professor Ross nor any of the discussions of that paper once alluded to the most important and best-established law of demography—that population is inversely proportional to intelligence. Of course there are other things of which the same general principle is true. Suicide, insanity, crime, and vice increase as we rise in the scale of intelligence. You do not find them among animals, and you find them less among savages and lower classes than in the upper strata of society. It is lowest in the scale of organic life that we find the highest fecundity, and the law goes back through the entire animal kingdom until we have those Protozoans in which one individual may be the parent of millions of offspring. This law also extends upward to the very topmost layers of society and finds its maximum expression

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[1] This word, so far as I am aware, has thus far appeared only three times in print, viz., first, in the paper of Professor J. Q. Dealey on "The Teaching of Sociology," read before the American Sociological Society, on December 31, 1909, and published in the *American Journal of Sociology* for March, 1910, XV, 662, and in the *Publications* of the society, IV, 182; second, in a review of Professor C. B. Davenport's *Eugenics*, by Mr. Carol Aronovici, in the *American Journal of Sociology* for July, 1910, XVI, 122; and, third, in Professor Dealey's recent work, *The Family in Its Sociological Aspect*, Boston, 1912, p. 128; each time in a footnote only, but also each time credited to Professor H. L. Koopman, Librarian of Brown University. Professor Koopman informs me that he suggested the word to Professor Dealey in conversation, and Professor Dealey admits this to be the origin of it. But the word seems to be needed, derived as it is from the Greek δῆμος, "the people at large," and signifying a science or doctrine of the welfare of the masses. It alliterates well with the other two words, "eugenics" and "euthenics," and yet it has a distinct meaning of its own, greatly expanding the whole field of discussion.

[2] See *Applied Sociology*, pp. 95–110, 129–81.

[3] Remarks on a paper by Professor D. Collin Wells on "Social Darwinism," read before the American Sociological Society on December 29, 1906. See the *American Journal of Sociology* for March, 1907, XII, 709–10, and the *Publications* of the society, I, 131–32.

in the very few who have attained to that lofty realm of wisdom where they not only understand the teachings of eugenics, but are capable of applying them to family life.

The doctrine defended by Professor Wells is the most complete example of the oligocentric world-view[1] which is coming to prevail in the higher classes of society, and would center the entire attention of the world upon an almost infinitesimal fraction of the human race and ignore all the rest. It is trying to polish up the gilded pinnacles of the social temple so as to make them shine a little more brightly, while entirely neglecting the great, coarse foundation-stones upon which it rests. The education and preservation of the select few, of the higher classes, of the emerged hundredth, to the neglect of the submerged tenth and the rest of the ninety-nine hundredths of society, covers too small a field. I cannot bring myself to work contentedly in a field so narrow, however fascinating in itself. Perhaps mine is a "vaulting ambition," but I want a field that shall be broad enough to embrace the whole human race.

For an indefinite period yet to come society will continue to be recruited from the base. The swarming and spawning millions of the lower ranks will continue in the future as in the past to swamp all the fruits of intelligence and compel society to assimilate this mass of crude material as best it can.

This is commonly looked upon as the deplorable consequence of the demographic law referred to, and it is said that society is doomed to hopeless degeneracy. Is it possible to take any other view? I think it is, and the only consolation, the only hope, lies in the truth that, so far as the native capacity, the potential quality, the "promise and potency" of a higher life are concerned, those swarming, spawning millions, the bottom layer of society, the proletariat, the working classes, the "hewers of wood and drawers of water," nay, even the denizens of the slums—that all these are by nature the peers of the boasted "aristocracy of brains" that now dominates society and looks down upon them, and the equals in all but privilege of the most enlightened teachers of eugenics.

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[1] *Applied Sociology*, p. 23.

Index

A star (*) indicates that the article is reprinted in the volume of the set indicated in this index.

A

*42. A Chapter of Law. I, 141
*82. A Congress of Birds. II, 56
*196. A Convenient System of River Nomenclature. IV, 2
507. A Famous Fossil Cycad. VI, 194
356. A Monistic Theory of Mind. V, 67
*203. A National University. IV, 20
*315. A National University, its Character. IV, 324
455. A New Compass Plant. VI, 47
*77. A New Fir of the Rocky Mountains. II, 32
446. A New Species of Eucalyptus from the Dakota Group of Southwestern Kansas. VI, 34
*80. A Party can Perpetuate its Rule. II, 52
*81. A Perpetual Oligarchy. II, 54
518. A Text-Book of Sociology. VI, 209
*34. About Evidence. I, 121
*15. Abraham Lincoln's Religion. I, 63
204. Administrative Report for the Year Ending June 30, 1883. IV, 23
207. Administrative Report for the Year Ending June 30, 1884. IV, 25
222. Administrative Report for the Year Ending June 30, 1885. IV, 72
262. Administrative Report for the Year Ending June 30, 1887. IV, 193
278. Administrative Report for the Year Ending June 30, 1888. IV, 234
296. Administrative Report for the Year Ending June 30, 1889. IV, 300
327. Administrative Report for the Year Ending June 30, 1890. IV, 335
352. Administrative Report for the Year Ending June 30, 1891. V, 34
370. Administrative Report for the Year Ending June 30, 1892. V, 135
393. Administrative Report for the Year Ending June 30, 1893. V, 241
404. Administrative Report for the Year Ending June 30, 1894. V, 259
420. Administrative Report for the Year Ending June 30, 1895. V, 320
443. Administrative Report for the Year Ending June 30, 1896. VI, 9
459. Administrative Report for the Year Ending June 30, 1897. VI, 55
462. Administrative Report for the Year Ending June 30, 1898. VI, 63
470. Administrative Report for the Year Ending June 30, 1899. VI, 101
474. Administrative Report for the Year Ending June 30, 1900. VI, 111
479. Administrative Report for the Year Ending June 30, 1901. VI, 116
488. Administrative Report for the Year Ending June 30, 1902. VI, 154
494. Administrative Report for the Year Ending June 30, 1903. VI, 165
512. Administrative Report for the Year Ending June 30, 1904. VI, 200
*292. Advertisement of Neo-Darwinism and Neo-Lamarckism. IV, 296
424. Age of the Island Series. V, 324
*39. Amende Honorable. I, 134
246. American Weather. IV, 141
514. An Example in Nomenclature. VI, 200
*202. An Example in Political Science. IV, 16
*167. An Interesting Botanic Relic of the District of Columbia. III, 298

*67. Announcement. I, 234
*521. Announcement of Applied Sociology. VI, 221
*431. Announcement of Dynamic Sociology, Second Edition. V, 321
*441. Announcement of Outlines of Sociology. VI, 7
*525. Announcement of The Psychic Factors of Civilization, Second Edition. VI, 239
*31. Another Pius Fraud. I, 113
199. Apparent Discontinuities in Biology. IV, 4
523. Applied Sociology. A Treatise on the Conscious Improvement of Society by Society. VI, 225
*325. [Appreciation of Childhood.] IV, 331
*561. Appreciation of: Sociological Study of the Bible by Louis Wallis. VI, 370
*468. Appreciation of the Monist and Open Court. VI, 99
*240. Asa Gray and Darwinism. IV, 116

B

*35. Beecher's Belief in Immortality. I, 123
*58. Benefit of Clergy. I, 210
*144. Bibliography for 1881. II, 143
*166. Bibliography for 1882. II, 292
*189. Bibliography for 1883. III, 412
*206. Bibliography for 1884. IV, 25
*217. Bibliography for the Six Months Ending June 30, 1885. IV, 46
*260. Bibliography for the Year Ending June 30, 1887. IV, 191
*282. Bibliography for the Year Ending June 30, 1888. IV, 239
*298. Bibliography for the Year Ending June 30, 1889. IV, 302
*329. Bibliography for the Eighteen Months from January, 1885, to June, 1886, and for the Year Ending June 30, 1890. IV, 336
*354. Bibliography for the Year Ending June 30, 1891. V, 35
*372. Bibliography for the Year Ending June 30, 1892. V, 138
*398. Bibliography for the Year Ending June 30, 1893. V, 246
*427. Bibliography for the Year Ending June 30, 1894, V, 325
*447. Bibliography for the Year Ending June 30, 1895. VI, 34
*226. Biographical Sketch. IV, 86
*194. Botanical View of the Fossil Flora of the Globe—Abstract. III, 432
*214. Broadening the Way to Success. IV, 31

C

306. Calcareous Algæ. IV, 317
*138. Captain C. E. Dutton on the Hawaiians. III, 118
*252. Carboniferous Glaciation. IV, 165
*123. Catalogue of a Collection of Japanese Woods. III, 62
*172. Caulinites and Zamiostrobus. III, 330
*254. Causes of Belief in Immortality. IV, 171
*87. Central Flowers of Daucus Carota. II, 145
*14. Charles H. Read and the Faculty of Harvard University. I, 59
*22. Christianity and Civilization. I, 82
*383. Christian Sociology. V, 194
*6. Circular of the National Liberal Reform League. I, 38
*155. Classification of Organisms. III, 254
*174. Clethra Alnifolia near Bladensburg, Md. III, 336
434. Collective Telesis. V, 336
*62. Comparative Theology. I, 222
480. Contemporary Sociology. VI, 117
*89. Cosmic and Organic Evolution. II, 148
363. Coxey's Army. V, 112
*267. Cretaceous Flora of Gay Head. IV, 220
*95. Cross-fertilization in Sabbatia angularis. II, 200

D

238. Dabney's Sensualistic Philosophy. III, 114
*76. Damiana.—Turnera Aphrodisiaca Ward. II, 27
*122. Darwin as a Botanist. II, 55
*160. Darwin's View of Christianity. II, 279
*509. Death of Gabriel Tarde. VI, 196
*140. Decumaria barbara. II, 126

*562. Définition du Progrès Sociale. VI, 372
*482. Definition of Sociology. VI, 126
433. Descriptions of the Species of Cycadeoidea . . . of Maryland. V, 335
457. Descriptions of the Species of Cycadeoidea. . .of the Black Hills. VI, 49
465. Description of a New Genus and Twenty New Species of Fossil Cycadean Trunks from the Jurassic of Wyoming. VI, 73
*162. Difference between the Latin and the Teutonic Races in Mixing with the Aborigines. II, 283
505. Discours de Clôture. VI, 193
502. Discours d'Ouverture. VI, 190
477. Discussion of La Mécanique Sociale. IV, 115
*225. Divine Origination of Matter or of Man. III, 81
*29. Doctrinal Sketches. [No. 1] Creation. I, 106
*32. Doctrinal Sketches [No. 2] The Fall of Man. I, 115
*36. Doctrinal Sketches. [No. 3] The Plan of Salvation. I, 124
*40. Doctrinal Sketches. [No. 4] The Atonement. I, 134
*46. Doctrinal Sketches. [No. 5] Change of Heart. I, 152
*49. Doctrinal Sketches. [No. 6] The Trinity. I, 162
*53. Doctrinal Sketches. [No. 7] Transubstantiation. I, 172
*60. Doctrinal Sketches. [No. 8] The Immaculate Conception. I, 215
*64. Doctrinal Sketches. [No. 9] The Incarnation. I, 226
*333. Dr. Newberry's Work in Paleobotany. IV, 373
450. Dr. Schenk's Method of Controlling Sex. VI, 37
145. Dynamic Sociology or Applied Social Science. II, 146

E

*350. Early Education and Precocity. V, 28
*84. Earthworms on the Pavements. II, 62
*75. Eccentricity of the Pith of Rhus Toxicodendron. II, 21
*18. Editorial. Frederick Douglass on the Churches. I, 72
*10. Editorial. Religious Bias of Judges. I, 50
*9. Editorial. The Present Age I, 48
*551. Education and Progress. VI. 330
209. Effect of Winds on the Diversion of Water Courses. IV, 28
491. Ehrenberg's Scientific Prediction. VI, 153
472. Elaboration of Fossil Cycads. VI, 107
*411. Ethical Aspects of Social Science. V, 269
*563. Eugenics, Euthenics, and Eudemics. VI, 382
239. Evidence of the Fossil Plants as to the Age of the Potomac Formation. IV, 114
*83. Evolution in Biology and Philology. II, 58
195. Evolution in the Vegetable Kingdom. IV, 1
515. Evolution of Social Structures. VI, 201
*109. Evolution of the Chemical Elements. II, 294
*374. Extract from a Letter Transmitting the Manuscript of "The Natural Storage of Energy." V, 165

F

*55. Faith or Fact. I, 202
*221. False Notions of Government. IV, 64
507. Famous Fossil Cycad. VI, 194
*105. Feeling and Function as Factors in Human Development. [Abstract.] II, 267
*106. Feeling and Function as Factors in Human Development. II, 269
*116. Field and Closet Notes on the Flora of Washington. II, 360
*181. Fish-Culture as an Example of Successful State Action. III, 386
266. Flora of the Laramie Group. IV, 220
*186. Fontaine's Older Mesozoic Flora in Virginia. III, 402
*253. Fortuitous Variation. IV, 167
362. Fossil Cycadean Trunks of North America. V, 111
388. Fossil Plants. V, 224
409. Fossil Plants of the Wealden. V, 268

342. Frost Freaks of the Dittany. IV, 380
*358. Frost Plants. V, 99

G

*57. Gag Law. I, 208
*268. Genius and Woman's Intuition. IV, 222
*134. Geographical Distribution of Human Races and Animals. II, 83
*330. Geographic Nomenclature of the Rock Creek Region. IV, 342
478. Geology of the Little Colorado Valley. VI, 116
*193. Geological View of the Fossil Flora of the Globe.—Abstract. III, 430
*177. Government Support of Irrigation Works in the West. III, 351
119. Guide to the Flora of Washington. II, 448

H

*85. Haeckel's Genesis of Man. I, 64
*367. Hegel on the State. V, 128
*500. Herbert Spencer's Autobiography. VI, 179.
*493. Herbert Spencer, his Death, his Visit to Washington, etc. VI, 160
*435. Herbert Spencer's Sociology. V, 341
*498. Herbert Spencer's Sociology. VI, 169
*192. Historical View of the Fossil Flora of the Globe—Abstract. III, 427
*100. Homologies in the Lauraceæ. II, 229
*48. Hope. I, 159
310. How the Missouri River Wears its Banks. IV, 319

I

*54. Immigration Statistics. I, 175
*110. Incomplete Adaptation in Plants. II, 315
210. Indians as Observers. IV, 28
430. Individual Telesis. V, 329
*132. Intellectual Work of Deaf-mutes. III, 82
*178. Irrigation in the Upper Missouri and Yellowstone Valleys. III, 353
*294. It Displeased the Czar. IV, 299

K

*120. Kant's Antinomies in the Light of Modern Science. III, 1

L

490. La Différenciation et l'Intégration Sociales. VI, 156
476. La Mécanique Sociale. VI, 112
*133. Language a Product of Intelligence. III, 82
534. La Sociologie des Partis Politiques. VI, 272
*44. Law. I, 148
454. L'Economie de la Douleur et l'Economie du Plaisir. VI, 45
*8. Legal Persecution. I, 47
484. [Le Matérialisme Historique.] VI, 128
520. Les Forces Phylogénétiques. VI, 220
533. Les Luttes Sociales et les Luttes Biologiques. VI, 269
*187. Lesquereux's Cretaceous and Tertiary Flora. III, 406
227. Lesquereux's Criticisms of the "Synopsis of the Flora of the Laramie Group" by Lester F. Ward, and the latter's reply thereto. IV, 86
504. Les Rapports entre la Sociologie et la Psychologie. VI, 191
513. Le Sujet Materière de la Sociologie. VI, 200
*114. Letter to John W. Mears. II, 354
*3. Letter to Mr. W. H. Thompson. I, 21
*4. Letter to Mr. W. H. Thompson. I, 29
*347. Letters Relating to the Genus Winchellia. IV, 384
*171. List of Plants added to the Flora of Washington from April 1, 1882, to April 1, 1884. III, 323
*124. List of Water Plants for Carp Ponds. III, 68
*449. Literary Note. [Outlines of Sociology.] VI, 36
503. L'Organisme Social. VI, 191
*550. Ludwig Gumplowicz. VI, 325

M

*529. Mars and its Lesson. VI, 244

*157. Marsh and Aquatic Plants of the Northern United States. III, 261
197. Meaning of the Word Nomadism. IV, 3
*68. Mental vs. Physical Liberty. I, 236
*516. Menthaceæ. VI, 206
*179. Mind as a Social Factor. III, 361
198. Mind as a Social Factor—Abstract. IV, 3
305. Miocene Plants from Northern Bohemia. IV, 316
497. Monogenism and Polygenism. VI, 168
*510. [Monument to Bernard Renault.] VI, 198
*201. Moral and Material Progress Contrasted. IV, 5
*212. Moral and Material Progress Contrasted—Abstract. IV, 29
541. [Motives in Social Conflict.] VI, 294

N

*396. Negative and Positive Wants. V, 243
*291. Neo-Darwinism and Neo-Lamarckism. IV, 253
*279. Nervation. IV, 235
375. Note on a Collection of Fossil Plants Made by Professor W. P. Jenney in the Black Hills. V, 165
*158. Note on a Collection of Plants from the Fort Union Group. III, 275
*159. Note on a Geological Excursion down the Missouri River. III, 277
558. Note on a Proposed Trip to Christiania, Hamburg, and Rome. VI, 361
*152. [Note on Gray and Trumbull's Review of Candolle's Origine des Plantes Cultivées.] III, 246
*526. [Note on Sympodial Plants.] VI, 241
495. Note on the Degree of Officer of Public Instruction of France. VI, 165
344. [Note on the Fossil Cycads from South Dakota.] IV, 382
*149. Note on the Preliminary Study of a Collection of Fossil Plants from the Lower Yellowstone. III, 241
211. Notes on the Flora of Eastern Virginia. IV, 28
336. Notice of Additions to the Paleobotany of the Cretaceous Formation on Staten Island; by Arthur Hollick. IV, 376
319. Notice of Albirupean Studies; by P. R. Uhler. IV, 329
264. Notice of a Memoir On Fossil Plants Collected by Mr. A. R. McConnell on Mackenzie River and by Mr. T. C. Weston on Bow River; by Sir William Dawson. IV, 197
307. Notice of a Memoir On the Fructification of Bennettites Gibsonianus Carr.; by H. Grafen zu Solms-Laubach. IV, 317
338. Notice of a Memoir On the Organization of the Fossil Plants of the Coal Measures, Part XIX; W. C. Williamson. IV, 377
286. Notice of a Memoir Ueber die Fructification von Bennettites Gibsonianus Carr.; von H. Grafen zu Solms-Laubach. IV, 244
271. Notice of a Memoir Ueber die Reste eines Brotfruchtbaums, Artocarpus Dicksoni n. sp., aus den Cenomanen Kreideablagerungen Grönland's; von A. G. Nathorst. IV, 231
324. Notice of a Memoir Ueber den Gegenwartigen Standtpunkt unseren Kenntniss von dem Vorkommen fossiler Glacialpflanzen. Von A. G. Nathorst. IV, 331
304. Notice of a Memoir Ueber Tertiärpflanzen von Chile; von H. Engelhardt. IV, 316
340. Notice of a New Taeniopterid Fern and its Allies; by David White. IV, 378
270. Notice of a Revision of the Genus Araucarioxylon of Kraus, with Compiled Descriptions and Partial Synonymy of the Species; by F. H. Knowlton. IV, 230
261. Notice of Contribuzioni alla Flora Fossile dei Terreni Terziarii della Liguria; by S. Squinabol. IV, 192
288. Notice of Das australische Florenelement in Europa; von Dr. Constantin Freiherrn von Ettingshausen. IV, 245
287. Notice of Die fossile Flora von Schönegg bei Wies in Steiermark; von Prof. Dr. Constantin

Freiherrn von Ettingshausen. IV, 244
*143. [Notice of Dynamic Sociology.] III, 134
*146. [Notice of Dynamic Sociology.] III, 232
*147. [Notice of Dynamic Sociology.] III, 235
*148. [Notice of Dynamic Sociology.] III, 238
*151. [Notice of Dynamic Sociology.] III, 244
335. Notice of Flora Tertiaria Italica. Auctoribus A. Meschinelli and X. Squinabol. IV, 376
337. Notice of Fossil Plants as Tests of Climate; by A. C. Seward. IV, 376
284. Notice of Handbuch der Palæontologie herausgegeben von Karl A. Zittel. IV, 243
256. Notice of Jurassic Plants from Kaga, Hida, and Echizen (Japan); by Matajiro Tokoyama. IV, 189
273. Notice of La Flora dei Tufi de Monti Somma; by Luigi Meschinelli. IV, 232
308. Notice of Le Nelumbium Provinciale des Lignites Crétacés de Fuveau en Provence; par le Marquis G. de Saporta. IV, 318
285. Notice of Monographie der Baltischen Bernsteinbäume; von H. Conwentz. IV, 243
277. Notice of Notes on the Leaves of Liriodendron; by Theodor Holm. IV, 234
242. Notice of Nya Anmärkningar om Williamsonia of A. G. Nathorst. IV, 139
321. Notice of Paléontologie Végétal (Ouvrages publiés en 1890), par R. Zeiller. IV, 329
*416. [Notice of Professor Giddings's Principles of Sociology.] V, 308
309. Notice of Recherches sur la Végétation du Niveau de Manosque; par le Marquis G. de Saporta. IV, 318
274. Notice of Remarks on Some Fossil Remains Considered as Peculiar Kinds of Marine Plants; by Leo Lesquereux. IV, 232
322. Notice of Sylloge Fungorum Fossilium hucusque Cognitorum. Auctore A. Meschinelli. IV, 330
272. Notice of Tertiäre Pflanzen der Insel Neusibiriens; von J. Schmalhausen. IV, 231
339. Notice of the Correlation of Early Cretaceous Floras in Canada and the United States; J. W. Dawson. IV, 377
320. Notice of the Fossil Flora of the Bozeman Coalfield; by F. H. Knowlton. IV, 329
236. Notice of the Memoir: Einleitung in die Paläophytologie vom botanischen Standtpunkt aus. Bearbeitet von H. Grafen zu Solms-Laubach. IV, 105
316. Notice of the Paleontology of the Cretaceous Formation on Staten Island. By Arthur Hollick. IV, 326
269. Notice of the Potomac or Younger Flora; by William M. Fontaine. IV, 230
276. Notice of the Revue des Travaux de Paléontologie Végétale, . . . par le Marquis Gaston de Saporta. IV, 233
*548. Notice of the Seventh Congress of the International Institute of Sociology. VI, 320
257. Notice of the Tertiary Flora of Australia; by Dr. Constantin, Baron von Ettingshausen. English translation, edited by R. Etheridge, Jr. IV, 190
323. Notice of I Tronchi di Bennettitee dei Musei Italiani. Notizie storiche, geologiche, botaniche; dei Professori Senatore G. Capellini e Conte E. Solms-Laubach. IV, 330
317. Notice of Untersuchungen über fossile Hölzer Schwedens; von H. Conwentz. IV, 326
289. Notice of Untersuchungen über Ontogenie und Phylogenie der Pflanzen auf Paläontologische Grundlage; von Prof. Dr. Constantin Freiherrn von Ettingshausen und Prof. Franz Krašan. IV, 245
229. Notice of Williamson's Memoir On the Organization of the Fossil Plants of the Coal Measures. Part XIII. IV, 89
235. Notice of Williamson's Memoir On the Organization of the Fossil Plants of the Coal Measures. Part XIV. IV, 104
*311. No Wonder it was Burned. IV, 319

O

*73. Oaks of the Potomac Side. II, 15
*45. Obituary of Mr. J. D. Gangewer. I, 150
*33. Official Bigotry. I, 118
*126. On Male Sexual Selection. III, 75
*170. On Mesozoic Dicotyledons. III, 306
*52. One Murderer in Hell. I, 169
*118. On the Cause of the Absence of Trees on the Great Plains. II, 439
213. On the Determination of Fossil Dicotyledonous Leaves. IV, 30
*125. On the Elimination of Unnecessary Grammatical Formalities. III, 74
*93. On the Genealogy of Plants. II, 174
*97. On the Natural Succession of the Dicotyledons. II, 210
*153. On the Position of the Gamopetalæ—Abstract. III, 248
*13. Organization. I, 56
*51. Organization. I, 167
*131. Origin of the Arabic Numerals. III, 82
275. Origin of the Plane-Trees. IV, 233
*38. Originality. I, 130
*241. Our Better Halves. IV, 127
183. Our Knowledge of Fossil Plants. III, 388
452. Outlines of Sociology. VI, 41

P

508. Paleozoic Seed Plants. VI, 195
*88. Pinus Pungens in the District of Columbia. II, 146
*142. Plant-life, Past and Present. III, 134
*391. Plutocracy and Paternalism. V, 228
*113. Politico-Social Functions. II, 334
*127. Politico-Social Functions—Abstract. III, 77
*94. Polypodium vulgare on Trees. II, 199
*343. [Preliminary Announcement of the Psychic Factors of Civilization.] IV, 381
*190. Premature Appearance of the Periodical Cicada. III, 417
*104. Pre-Social Man — Abstract. II, 265
*111. Pre-Social Man. II, 324
302. [Principes de Corrélation Géologique au Moyen de Plantes Fossiles.] IV, 308
364. Principes et Méthodes d'Etude de Corrélation Géologique au Moyen de Plantes Fossiles. V, 112
299. Principles and Methods of Geologic Correlation by Means of Fossil Plants. IV, 305
313. Principles and Methods of Geologic Correlation by Means of Fossil Plants—Abstract. IV, 322
295. [Principles of Geologic Correlation by Means of Fossil Plants —Abstract.] IV, 300
432. Professor Fontaine and Dr. Newberry on the Age of the Potomac Formation. V, 332
*407. Professor Giddings's Principles of Sociology. V, 263
*169. Prof. Sumner's Social Classes. III, 301
*141. Proterogyny in Sparganium eurycarpum. III, 128
448. Psoralea tenuiflora as a Tumble-weed. VI, 36
487. Pure Sociology. VI, 138

Q

*107. Quercus Leana, Nutt. II, 276

R

2. Rapidity of Progress. I, 16
368. Recent Discoveries of Fossil Cycadean Trunks in the Potomac Formation of Maryland. V, 133
425. Reconnaissance through Indian Territory, Oklahoma, and Southwestern Kansas. V, 324
384. Relation of Sociology to Anthropology. V, 195
*536. Relations of the Social Sciences. VI, 274
*23. Religion and Progress. I, 85
*28. Religious Influence of Science. I, 102
*63. Religious War. I, 224
*248. Remarks on an Undescribed Vegetable Organism from the Fort Union Group of Montana. IV, 149
*92. Remarks on Texas. II., 172

*382. Remarks on the Genus Caulinites. V, 192
205. Report of the Curator of the Department of Fossil Plants of the U. S. National Museum for the Year 1884. IV, 23
*165. Report of the Department of Fossil Plants of the U. S. National Museum for the Year Ending June 30, 1882. III, 289
243. Report on the Department of Fossil Plants in the United States National Museum for the Year Ending June 30, 1886. IV, 139
258. Report on the Department of Fossil Plants in the United States National Museum [for the Year Ending June 30], 1887. IV, 190
280. Report on the Department of Fossil Plants in the United States National Museum [for the Year Ending June 30], 1888. IV, 238
328. Report on the Department of Fossil Plants in the United States National Museum for the Year Ending June 30, 1890. IV, 335
353. Report on the Department of Fossil Plants of the United States National Museum [for the Year Ending June 30], 1891. V, 34
371. Report on the Department of Fossil Plants in the United States National Museum [for the Year Ending June 30], 1892. V, 136
397. Report on the Department of Fossil Plants in the United States National Museum for the Year Ending June 30, 1893. V, 246
426. Report on the Department of Fossil Plants in the United States National Museum for the Year Ending June 30, 1894. V, 325
216. Report on the Department of Plants in the United States National Museum for the Six Months Ending June 30, 1885. IV, 44
244. Report on the Department of Recent Plants in the United States National Museum for the Year Ending June 30, 1886. IV, 139
259. Report on the Department of Recent Plants in the United States National Museum, [for the Year Ending June 30], 1887. IV, 190
281. Report on the Department of Recent Plants in the United States National Museum [forthe Year Ending June 30], 1888. IV, 238
215. Report on the Departments of Fossil and Recent Plants of the United States National Museum [for the Six Months Ending June 30], 1885. IV, 43
*188. Report on the Fossil Plants of the United States National Museum for 1883. III, 410
*467. Report on the Petrified Forests of Arizona. VI, 76
*184. Report to the Director of the United States Geological Survey for the Year 1881–1882. III, 395
*341. [Resolutions on the Death of Dr. George Vasey.] IV, 378
*25. Revealed Religion and Human Progress. I, 91
475. Review of a Memoir Sur Quelques Microorganismes des Combustibles Fossiles; par. B. Renault. VI, 111
415. Review of An Examination of Weismannism; by George John Romanes. V, 308
517. Review of An Outline of the Theory of Organic Evolution; by Maynard M. Metcalf. VI, 208
*156. Review of Chapters XI.–XIV. of Dynamic Sociology. III, 256
297. Review of Fossil Botany, being an Introduction to Paleophytology from the Standpoint of the Botanist. By H. Graf zu Solms-Laubach. IV, 301
456. Review of Fossil Plants for Students of Botany and Geology. By A. C. Seward. VI, 48
486. Review of Homeric Society; by Albert Galloway Keller. VI, 137
389. Review of La Recherche del Unité; by E. de Roberty. V, 226
410. Review of Lecky's Democracy and Liberty. V, 268
423. Review of L'Ethique—Le Bien et le Mal. Par E. de Roberty. V, 323
438. Review of Organisme et Société; par René Worms. VI, 2

*412. Review of Professor Giddings's Principles of Sociology. V, 282
466. Review of Social Laws; by G. Tarde. Translation from the French by Howard C. Warren. VI, 74
*543. Review of Social Psychology; by Edward Alsworth Ross. VI, 296
419. Review of Social Rights and Duties. By Leslie Stephen. V, 319
528. Review of Sociology and Social Progress. Compiled by Thomas Nixon Carver. VI, 243
559. Review of The Changing Chinese. By Edward Alsworth Ross. VI, 362
360. Review of The Distribution of Wealth; by John R. Commons. V, 109
232. Review of The Geological History of Plants. By Sir William Dawson. IV, 92
511. Review of The Jurassic Flora. By A. C. Seward. VI, 199
*414. Review of The Life and Letters of George John Romanes, M. A., LL.D., F.R.S. Written and Edited by his Wife. V, 306
496. Review of The Positive Philosophy of Auguste Comte; by L. Lévy-Bruhl. VI, 166
*408. Review of The Primary Factors of Organic Evolution. By E. P. Cope. V, 266
547. Review of The Problem of Age, Growth, and Death. By Charles S. Minot. VI, 319
*47. Review of The Science of Evil, or First Principles of Human Action. By Joel Moody. I., 156
*436. Review of the Science of Mythology. V, 344
453. Review of The Social Mind and Education. By George Edgar Vincent. VI, 44
469. Review of The Theory of the Leisure Class. By Thorstein Veblen. VI, 100
403. Review of The Theory of Social Forces. By Simon N. Patten. V, 258
458. Review of Truth and Error or the Science of Intellection. By J. W. Powell. VI, 51
499. Review of Where did Life Begin? By Gilbert Hilton Scribner. VI, 178
*71. Rocky Mountain Flora. II, 1
394. Romanes on Darwinism. V, 242

S

*386. Saporta and Williamson and their Work in Paleobotany. V, 205
*112. Savage and Civilized Orthoepy. II, 328
*219. Science and Immortality. IV, 56
*150. Science and the Use of Capitals. III, 242
*12. Science vs. Theology. I, 53
*121. Scientific Basis of Positive Political Economy. III, 18
*37. Scraps of Ecclesiastical History. No. 1. The Sacred Writings. I, 127
*41. Scraps of Ecclesiastical History. No. 2. The New Testament. I, 137
*50. Scraps of Ecclesiastical History. No. 3. Church Councils. I, 164
*61. Scraps of Ecclesiastical History. No. 4. Church Councils (*cont.*). I, 218
*65. Scraps of Ecclesiastical History. No. 5. Persecution of the Albigenses. I, 229
*70. Scraps of Ecclesiastical History. No. 6. Persecution of the Waldenses. I, 242
*524. Second Announcement of Applied Sociology. VI, 236
*346. [Second Announcement of The Psychic Factors of Civilization.] IV, 383
*99. Second Blooming. II., 223
251. Seventh Annual Report of the Geological Survey, 1885–1886. IV, 165
*102. Sexual Differentiation in Spigæa repens. II, 243
*43. Sin. I, 145
*117. Sketch of John W. Powell. II, 426
208. Sketch of Paleobotany. IV, 26
535. Social and Biological Struggles. VI, 273
538. Social Classes in the Light of Modern Sociological Theory—Abstract. VI, 291
540. Social Classes in the Light of Modern Sociological Theory. VI, 293
532. Social Darwinism. VI, 268

489. Social Differentiation and Social Integration. VI, 154.
200. Social Forces. IV, 3
528. Social Genesis. V, 327
442. Social Integration through Conflict. VI, 295
*163. Society as a Domain of Natural Forces. III, 285
*369. Sociology [A Definition]. V, 134
399. Sociology and Anthropology. V, 250
392. Sociology and Biology. V, 241
390. Sociology and Cosmology. V, 227
460. Sociology and Economics. VI, 55
402. Sociology and Psychology. V, 258
553. Sociology and the State. VI, 350
*485. Sociology as a Science. VI, 128
481. Sociology at the Paris Exposition of 1900. VI, 121
555. [Solidarité Sociale.] VI, 357
421. Some Analogies in the Lower Cretaceous of Europe and America. V, 320
*250. Some Social and Economic Paradoxes. IV, 153
*552. Spain and Ferrer. VI, 341
*233. Special Weather Bulletin. IV, 92
*361. Spencer-smashing at Washington. V, 109
*98. St. Louis and Botany. II, 223
*381. Static and Dynamic Sociology. V, 176
471. Status of the Mesozoic Floras of the United States. First Paper. VI, 102
519. Status of the Mesozoic Floras of the United States. Second Paper. VI, 213
*357. Status of the Mind Problem. V, 69
*175. Sweet Cicely as a Bur. III, 337
130. Symbolic Interpretation. III, 81
223. Synopsis of the Flora of the Laramie Group. IV, 72

T

*96. Texas Plants. II, 203
*56. The Analogy of Religion. I, 205
*115. The Animal Population of the Globe. II, 356
*35. The Anthropocentric Theory. III, 85
*161. The Appraisement of Literature. III, 282
*473. The Autochthonous or Allochthonous Origin of the Coal and Coal Plants of Central France. VI, 109
*21. The Bible vs. Slavery. I, 80
*545. The Career of Herbert Spencer. VI, 301
*173. The Claims of Political Science. III, 333
*265. The Course of Biologic Evolution. IV, 198
440. The Cretaceous Formation of Kansas. VI, 6
463. The Cretaceous Formation of the Black Hills as Indicated by the Fossil Plants. VI, 64
365. The Cretaceous Rim of the Black Hills. V, 115
406. The Data of Sociology. V, 263
492. The Dresden Cycad. VI, 159.
*16. The Entering Wedge. I, 66
*444. The Essential Nature of Religion. VI, 9
527. The Establishment of Sociology—Abstract. VI, 242
*530. The Establishment of Sociology. VI, 259
*230. The Evolution of Immortality. IV, 89
*359. The Exemption of Women from Labor. V, 100
*401. The Filiation of the Sciences. V, 252
531. The Fine Arts as a Dynamic Factor in Society. VI, 266
*180. The Fossil Flora of the Globe. III, 378
*464. The Fossil Forests of Arizona. VI, 71
*231. The Frequency of Coincidences. IV, 91
*549. The Geneva Jubilee. VI, 321
263. The Geographical Distribution of Fossil Plants. IV, 193
*191. The Ginkgo-tree. III, 421
300. [The Glossopteris Flora.] IV, 306
*461. The Gospel of Action. VI, 56
557. The Hierarchy of the Sciences. VI, 360
*554. The Historical View of Woman. VI, 353

*220. The Immortality that Science Teaches. IV, 59
*445. The Increase and Diffusion of Knowledge among Men. VI, 33
*247. The King-Devil. IV, 142
*228. The Law of Malthus. IV, 87
*78. The Local Distribution of Plants and the Theory of Adaptation. II, 37
418. The Mechanics of Society. V, 318
377. The Mesozoic Flora of Portugal Compared with that of the United States V, 173
*442. [The Mesquite in Southern Kansas.] VI, 8
*400. The Metaphysics of Romanes. V, 250
*373. The Natural Storage of Energy. V, 142
*439. The Nature of Pleasure. V, 4
*332. The New Botany. IV, 367
*26. The New Faith. I, 96
385. The Nomenclature Question. V, 197
*72. The Oaks of the District. II, 13
*136. The Organic Compounds in their Relations to Life. III, 97
*154. The Organic Compounds in their Relations to Life—Abstract. III, 252
245. The Paleontologic History of the Genus Platanus. IV, 140
249. The Paleonotologic History of the Genus Platanus. IV, 152
522. The Past and Future of the Sexes. VI, 223
387. The Place of Sociology among the Sciences. V, 218
301. The Plant-Bearing Deposits of the American Trias. IV, 306
312. The Plant-Bearing Deposits of the American Trias — Abstract. IV, 321
*234. The Political Ethics of Herbert Spencer. IV, 375
*355. The Political Ethics of Herbert Spencer. V, 38
*24. The Polygamy Discussion. I, 88
*139. The Postage Question. III, 123
405. The Potomac Formation. V, 260
*90. The Province of Statistics. II, 164
349. The Psychic Factors of Civilization. V, 1
*331. The Psychologic Basis of Social Economics. IV, 345
506. The Pteridospermaphyta. VI, 194
*5. The Punishment of Traitors. I, 36
422. The Purpose of Sociology. V, 323
*326. The Reciprocal Obligations of Parents and Children. IV, 322
*103. The Relation between Insects and Plants. II, 248
379. The Relation of Sociology to Economics. V, 174
501. [The Rise of the Proletariat.] VI, 188
*30. The Rising School. I, 110
*429. [The Ruralization of City Populations.] V, 327
*66. The Sabbath Question. I, 231
*314. The Science and Art of Government. IV, 322
*7. The Situation. I, 42
345. The Social Effects of Taxation. IV, 382
*69. The Social Evil. I, 238
413. The Social Forces. V, 305
*164. The Sociological Meaning of Benevolent Institutions. III, 187
*255. The Sociological Position of Protection and Free Trade. IV, 180
539. The Sociology of Political Parties. VI, 300
*1. The Spaniard's Revenge. I, 1
546. The Status of Sociology. VI, 317
*560. The Struggle of To-day. VI, 364
*290. The Transmission of Culture. IV, 246
378. [The True Rôle of Competition.] V, 174
*283. The Unearned Increment of Wealth. IV, 241
*176. The Upper Missouri River System. III, 340
*218. The Use and Abuse of Wealth. IV, 47
*303. The Utilitarian Character of Dynamic Sociology. IV, 309
544. The Value of Error. VI, 300
*91. The Way to Scientific Law-making. II, 168
483. The Word "Ecology." VI, 127
*108. The World's Supply of Live Stock. II, 280
*351. Third Announcement of the Psychic Factors of Civilization. V, 32

*101. Timber Trees and Grasses of Utah. II, 235
*86. Timber Trees of the Dismal Swamp. II, 141
*79. Tipularia discolor. II, 50
*237. True and False Civil Service Reform. IV, 105
*185. Twofold Drainage of the Dismal Swamp. III, 401
*293. Two Views of Political Economy. IV, 297
224. Types of the Laramie Flora. IV, 78

U

451. Utilitarian Economics. VI, 38

V

*376. Vegetation of the Ancient World. V, 166
*128. Virtual and Nominal Chiefs. III, 80
*168. Vital Force. III, 300
395. Voluntary Organization. V, 242
*417. Voluntary Organization. V, 313
*129. Vowel Systemization. III, 181

W

556. [War and Progress.] VI, 359
*59. Washington Items. I, 212
366. Weismann's Concessions. V, 117
*318. Weismann's New Essays. IV, 327
*348. Weismann's Theory of Heredity. IV, 385
437. What Are Stipules? VI, 1
*537. What Brings out Genius? VI, 277
*11. What does it Prove? I, 51
*19. What has been Gained? No. 1. I, 74.
*20. What has been Gained? No. 2. I, 77
*27. What Is the Positive Philosophy? I, 99
*137. What Mr. Ward was Ready to Say. III, 112
*234. What shall the Public Schools Teach? IV, 96
*17. Who Destroyed the Alexandrian Library? I, 69
*182. Why is Water Considered Ghost-proof? III, 387
*380. William Bower Taylor. V, 175
*74. Winter-blooming Jessamine. II, 19

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